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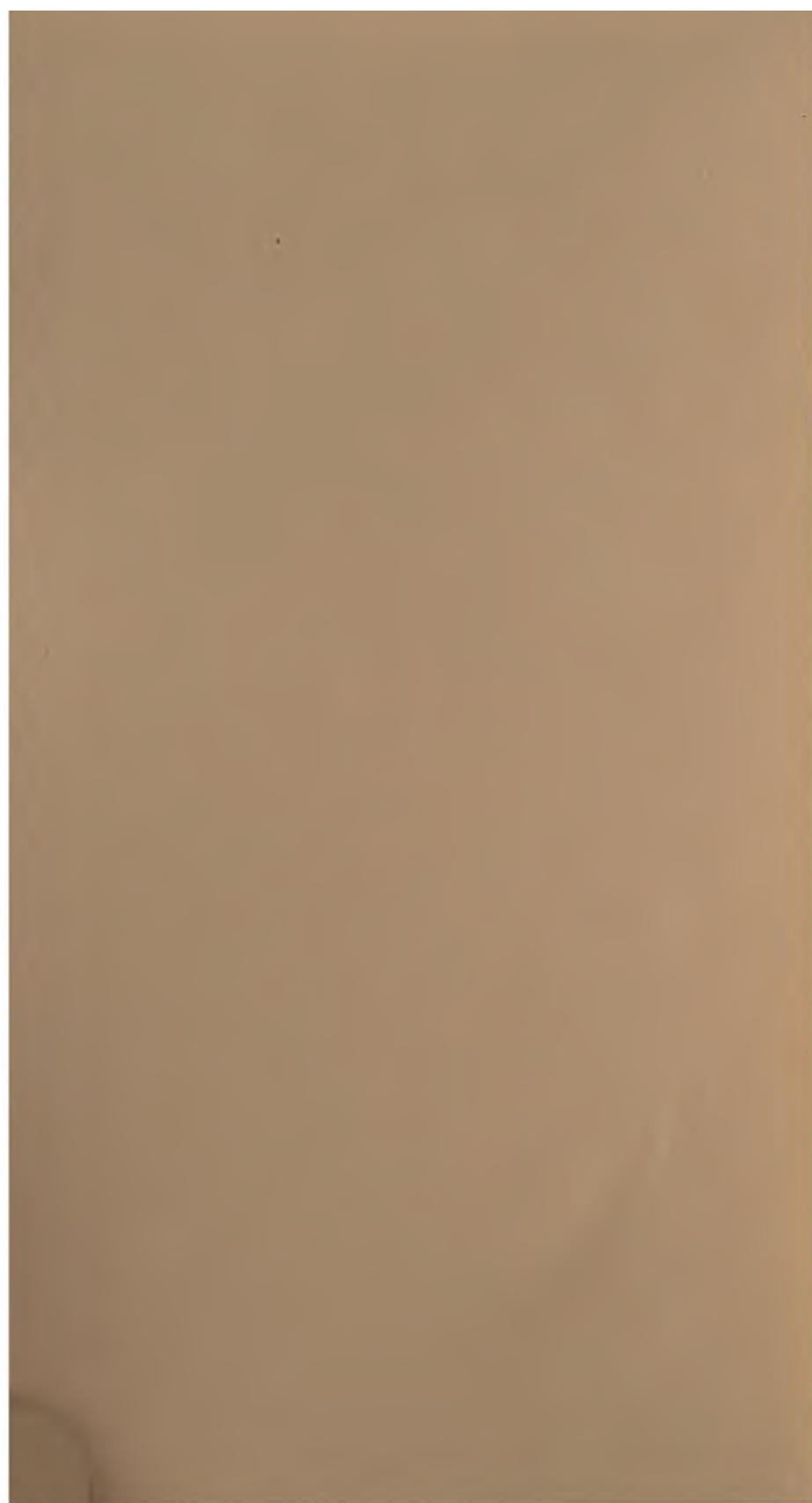
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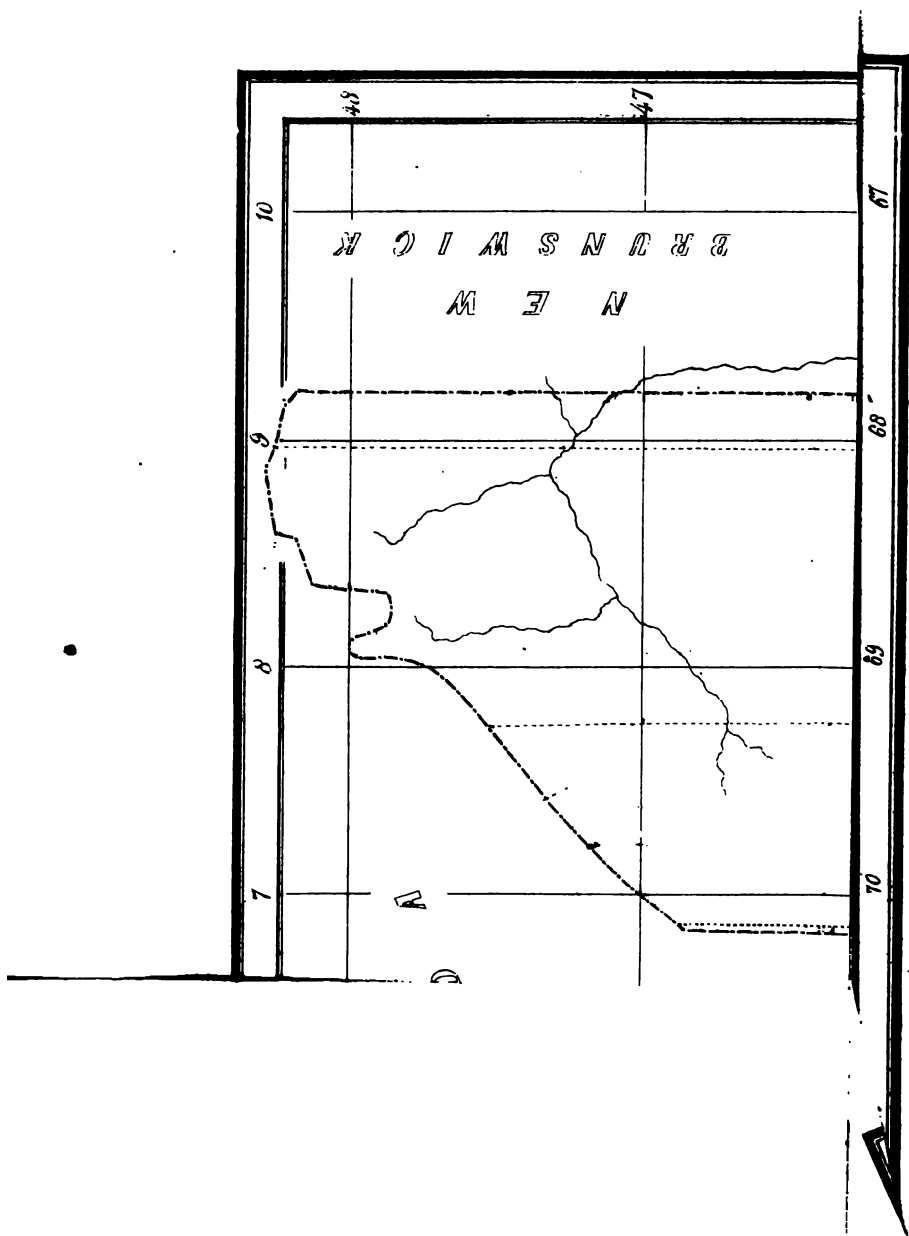








BOYLSTON PRIZE DISSERTATIONS.



BOYLSTON

PRIZE DISSERTATIONS

FOR THE YEARS 1836 AND 1837.

BY OLIVER WENDELL HOLMES, M. D.,

FELLOW OF THE MASSACHUSETTS MEDICAL SOCIETY, AND MEMBER OF THE
SOCIÉTÉ MÉDICALE D'OBSERVATION OF PARIS.

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TO

P. CHA. A. LOUIS,

DOCTOR IN MEDICINE OF THE FACULTIES OF PARIS AND ST. PETERSBURG,
PRESIDENT OF THE SOCIÉTÉ MÉDICALE D'OBSERVATION, ETC.

IN THE RECOLLECTION OF HIS INVALUABLE INSTRUCTIONS

AND UNVARYING KINDNESS,

THESE ESSAYS ARE RESPECTFULLY

INSCRIBED.

P R E F A C E .

AN author owes it to himself, upon coming before the public, to assume an attitude of which he need not be ashamed, whatever may be the opinions formed respecting his work. In the warmth of composition, or the ardor of controversy, he may have often betrayed prejudice and undue self-confidence ; he may have erred, even while engaged in detecting error, and have injured the interests of truth amidst his efforts to strengthen her cause. I would offer a few prefatory remarks, not merely in courtesy to my reader, but in justice to the feelings with which I regard this volume, now completed, and about to be sent abroad. While it is yet within my own hands, let me be the first to stamp my work with the inevitable mark of imperfection.

I am conscious that the Essays contained in this volume do but partial justice to the subjects of which they treat. Two of them, those upon Neuralgia and Direct Exploration, were necessarily written within a very limited period ; the other engaged my attention for a much longer time. I shall say a few words respecting each of them in the order in which they are arranged.

When the question respecting the Indigenous Intermittent Fever of New England first met my eye, I was disposed to consider the investigation as promising little to any one, and

least of all to myself, recently established in the medical profession, little familiar with our local medical history, and having few acquaintances beyond my immediate vicinity. The first researches which I made were little satisfactory, and it was only after a considerable time that I was convinced the subject offered any reasonable motive to continue. I am fully aware that this dissertation contains much that is arid and wearisome ; that its pretensions are of the humblest nature, as presenting little more than the details of facts, and that very often in the language of others ; and yet I wish to have it understood that it required far more time and patience than both the others united. Those who glance their eye over a few pages of historical research are very apt to deceive themselves as to the efforts which they cost the author, and if I must own that I have been often tedious, and always unambitious in this dissertation, I must be permitted to say that those who judge of the labor by the results will perhaps do me injustice.

The Essay on Neuralgia contains ideas and arguments from which many of its readers will necessarily differ. I can only say that in citing opinion after opinion from others, in order to examine and reject them, I have followed the plain dictates of my reason, and it is only in two or three instances that I have been conscious of any feelings or prejudices about the matter in dispute ; which I leave to the reader's sagacity to discover, only suggesting that a writer's dislikes and partialities respecting doctrines and dogmatists have little to do with his arguments, and sometimes add vivacity to a dull discussion.

The Essay on Direct Exploration has been once printed, and presented by the munificence of an individual, under the auspices of the Massachusetts Medical Society, to all the

members of that Association. I cannot but mention this circumstance with satisfaction, and express the wish that it had been worthier of this honor. In presenting it once more, with some corrections and alterations, to the public, I have been partly influenced by the desire of bringing all my productions of this nature into a single volume. No one is more thoroughly aware than myself of the deficiencies of this dissertation. To answer the question satisfactorily, it would be necessary to pass some years in a hospital, examining every case through the whole series of medical diseases, by all the physical and all the rational signs, and by an exact analysis of these cases to decide the question of their relative importance and utility.

I have throughout the volume subjoined my authorities at the foot of the page, preferring the charge of pedantry to that of vagueness and inaccuracy, and having learned from experience the value of such references. For whatever may be rash and erroneous in these treatises, the recorded vote of the Boylston Medical Committee, "That the Board do not consider themselves as approving the doctrines contained in any of the Dissertations to which the premiums may be adjudged," renders me alone responsible. It is not my present intention to become a candidate for these honors hereafter, and I could not find a fitter occasion to express the feelings of gratitude with which all true lovers of Science, and especially those who have received the rewards his liberality provided, must regard the name of that generous patron of medical learning with whose honored memory it is my pleasure as well as my duty to associate whatever these pages may contain of interest or utility.

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FACTS AND TRADITIONS
RESPECTING THE EXISTENCE OF
INDIGENOUS INTERMITTENT FEVER
IN NEW ENGLAND.

Perseverando.

DISSERTATION.

"TO WHAT EXTENT, AND IN WHAT PLACES HAS INTERMITTENT FEVER
BEEN INDIGENOUS IN NEW ENGLAND?"

As the question proposed by the Committee is a purely historical one, and therefore to be answered by presenting the greatest practicable number of facts, attested as well as circumstances allow, and arranged in the most connected order to which they can be reduced, I may proceed with little ceremony to thrust in my sickle upon the scanty harvest before me. I shall only ask for a few moments to consider both the favorable and forbidding aspects of the question; to point out the plan which I intend to follow; to mention a few of the laws ascertained by experience with regard to the production of the disease in question; and pass to the humble task which I have undertaken, perhaps unconscious of the weariness and vexations with which it may be attended.

I may remark, in the first place, that the striking character of the disease renders it a favorable subject for the medical historian. He may indeed be deceived as to the place of its origin, for it often remains latent in the system for many months, perhaps, in

some instances, for years. It is by no means uncommon to meet with cases of intermittent fever in the most salubrious districts of New England, in persons who have been residing for a considerable time at a distance from any known source of Malaria. But we generally find, that such individuals have been, within the course of the year, in places where the disease is acknowledged to be endemic; in the Western States; very frequently in the western parts of New York; sometimes in regions still more remote, as in a case lately under my own care, where the patient had contracted the affection in the West Indies. To this circumstance in the history of the disease, most of my correspondents have alluded, and it is so generally known, that such cases are almost always referred to their true origin. This single source of fallacy being removed, there remains little room for error. The rudest observer can hardly overlook the periodical returns of the febrile paroxysm, the manifestation of that wonderful law which seems to change his organs into clockwork, to measure the mysterious revolutions and phases of an unknown moving cause; and thus even vulgar tradition becomes of value in relation to phenomena so palpable to the simplest apprehension.

And if this single fact, the definite nature of the disease, be almost the only encouraging point in the question proposed, I cannot but think, that with all its difficulties, and in spite of its apparent sterility, it may well excite a deep and serious interest.

It does not call for ingenious speculations, which may at any time be annihilated by some unforeseen discovery, or thrown by for some more captivating

novelty. It does not ask for a few notions relating to some disease which future observation may contradict, or explain or extend, until they lose their meaning. It calls on some quiet student, who has a little time, a little patience, a little opportunity, to rescue from forgetfulness a series of facts, which but for him may perish from the memory of men. Many of the lips from which he is to learn, will soon be closed for ever; the gray-haired fathers of his art will soon be laid down to rest in the valleys and by the streams, and on the hillsides, whose simple annals their memories preserved; new, and perhaps wiser men may take their places, but who can fill the records which their trembling hands have left untraced? It is this feeling which encourages me, as it may some other more competent to the task, to enter the arena with the closed vizard and unblazoned shield of an anonymous competitor.

However imperfectly I may succeed in my attempt, that attempt must be useful, by marshalling some of the scattered facts from which the medical topography of New England is to be framed. It will be useful by showing that something may be gleaned by the humblest laborer in this scanty field, and not the less so, if another should return more richly laden. It will be useful, if destined to be known, by affording a nucleus around which similar facts may be symmetrically arranged. It will be of peculiar utility, in contributing to excite an interest in the members of the profession, in the endemics belonging to different regions of New England; the want of which interest leaves so many chasms in the path of the medical historian.

But it is in vain to deny that the investigation is surrounded with difficulties. There was no Sydenham among the pilgrims to follow the atmospheric constitution through all its changes, and transmit the very lineaments and expression of diseases to their children. While their ecclesiastical councils were in solemn debate upon monstrous births¹ and signal providences, the outlines of our early medical history faded away almost unnoticed. Something has indeed been accomplished in more recent times, in the local histories which have been published, but these are generally vague and imperfect, and are but as a few grains of sand sprinkled over the map of a vast and varied territory. A still more limited number of local descriptions, expressly medical in their character, has testified that a few observing minds have felt the value and interest of such labors to the present and the future ; we can only regret that so few have thus contributed to the common store of science. The entire apathy with which the late attempt of a Committee of the Massachusetts Medical Society, appointed "to investigate the History of Intermittent Fever in the Commonwealth of Massachusetts, and throughout New England generally," was received, is by no means encouraging to such as venture upon the same field of investigation. A circular, prepared by this Committee "was communicated to all the members of this Society, and to a considerable number of physicians in the other New England States directly ; and was also published extensively in the medical

¹ See the case of Mary Dyer, in Winthrop's Journal, for an appalling description of a few hydatids and coagula.

journals and newspapers in various parts of the country." "In answer to this application the Committee received communications from but two individuals, both stating facts of importance, but only one of them entering into any detailed reply to the questions proposed."¹ Having failed to procure materials, they requested and received a discharge from the farther consideration of the subject. Such is the aspect of the question, definite in its intention, interesting and important in its final object, sterile and repulsive in its prospects.

I proceed to mention the plan which I intend to follow in the pages strictly devoted to answering the question proposed. The poverty and looseness of our early histories, so far as they relate to this point, has not prevented me from wading through them with more care and labor than they perhaps deserved. I shall arrange whatever I have found in their columns under the general head—*Testimony of the Earlier Writers on New England*. The facts, much more ample and satisfactory, though still very imperfect, which I have obtained from more recent sources, in different methods, I shall dispose according to a geographical order. As the outlines of the States are for the most part arbitrary divisions, it seems more expedient to follow this plan, than to separate a series of facts connected by a common cause, which is totally indifferent to State rights and factitious boundaries. I shall, then, bring forward whatever evidence I may find, in the following order :

¹ Med. Com. of the Mass. Med. Soc. Vol. V, part VI, Appendix.

1. Facts relating to localities upon the Atlantic shore, and its inlets.
2. Those relating to the places situated upon the larger rivers, and their tributaries.
3. Those relating to places bordering upon lakes and ponds.
4. Any facts which do not fall within these divisions.

The terms of the question, "To what extent," are sufficient to justify the introduction of negative evidence. This will generally be of such a nature as to authorize a presumptive conclusion, rather than to afford peremptory proof; which, indeed, could hardly be obtained with regard to any place, unless the graves of its settlers and its patriarchs could restore the experience and the traditions which slumber with them in the churchyard. If, in the words of a recent author,¹ "it must be plain that to detail the geography of Malaria for the whole world, would be little else than to write a general grammar of geography," the same remark is true with regard to any part of the world, and applies to the region under consideration. But however desirable, this cannot at present, if it ever can be, accomplished with regard to New England, and we must proceed, like the surveyor, by means of a few fixed points and angles, to approximate, as well as we may, to the solution of our problem.

We come to the consideration of some of the laws which regulate the production of intermittents, as these laws are applicable to New England. I need not insist on the character of soil known to be ca-

¹ Macculloch, *Essay on Malaria*, Philad. 1839.

pable of generating these affections, nor on the marshes and swamps, and lakes, and rivers, which so abundantly diversify the surface of the Eastern States. If the salt marshes of Normandy,¹ the "pond occupying an old gravel pit" at Woolwich,² and the canal which ornaments the royal gardens of Versailles,³ have all been scandalously productive of intermittents, how few of the villages of New England but have spots a thousand times more open to suspicion! And in addition to the standing causes in the nature of the soil, the extensive inundations which so frequently take place upon the rivers of this region, might seem almost enough of themselves to fill its hospitals annually with the diseases produced by Malaria.

The New England States, included between the latitudes 41° and 48°, correspond in their different parallels with the pestilential districts of France and Italy, and are far to the south of other countries commonly affected with intermittents, as England and Holland. Nay, even in a much higher latitude, twelve degrees to the north of the most remote extremity of Maine, the city of Stockholm "is familiarly known for the severity and inveteracy of its intermittents."⁴ And in our own continent the very general prevalence of the same fevers in the parallel of New England, to the west of the chain of mountains by which it is traversed, both in the United States and in Upper Canada, is too well known to require any illustration.

Again, as the production of Malaria, and the origin

¹ Macculloch, *Essay*, etc. p. 21.

² *Ibid.* p. 53.

³ *Ibid.* p. 54.

⁴ *Ibid.* p. 189.

of intermittents is dependent within certain limits upon temperature, we may devote a few lines to that of the New England climate. The author whom I have repeatedly cited, and who has treated the subject of Marsh Miasmata more elaborately and faithfully than any other English author with whom I am acquainted, remarks that "Malaria may be expected during the warm season, and particularly under the various circumstances of heat and moisture formerly discussed, in every country in which the mean annual temperature is 45, or even less, much more certainly, when that reaches to 50, and most indubitably when it exceeds that in all such places as the following,"—which are, alluvial districts attending rivers, whether inland or on the seashore, valleys liable to the accumulation of water, and the margins of lakes. By the side of this result of a competent observer, I will place such estimates of the mean annual temperature of different Eastern States as I have been able to obtain.

Maine,	Eastport,	41½ to 42.	Am. Alm. for 1836.
New Hampshire.		46, 1.	{ Wilson's table. Inquiry into the Nature and Treatment of Spotted Fever, &c.
" "	Amherst,	52, 20.	{ John Farmer Hist. Coll. 2d series, VII, 67.
" "	Salisbury,	53, 25.	{ Farmer.
Vermont.		43, 3.	{ Wilson's table.
Massachusetts.	Salem,	48, 86.	{ Dr. Holyoke. Am. Alm. 1837.
Rhode Island.	Providence,	47, 23.	{ Prof. Caswell. Am. Alm. 1837.

According to Dr. Lovell's meteorological observations (Philad. Med. Journal, Vol. IX, p. 464), the mean temperature for a year, at four of the New England Atlantic stations, namely, Eastport and Portland, Maine, Fort Constitution, near Portsmouth, N.H., and Newport, R. I., was 47, 23.

And since, at four Western stations, about corresponding in latitude to those just mentioned, namely, Fort St. Anthony, Green Bay, Prairie du Chien, and Council Bluffs, the mean annual temperature was only 46, 04, or more than a degree less, while, according to the army returns, "at the Western ports, by far the greater proportion of cases are intermittents," and the district intervening between these stations is full of similar cases, we are led to conclude that the exemption of New England, to a considerable extent, from intermittents, cannot be traced to the low standard of its mean annual temperature.

We shall have occasion to witness, in different places, the disappearance of fever and ague, in consequence of agricultural and other changes, artificially produced. But it is obvious that the New Englander does not principally owe his comparative immunity to his greater industry and skill in cultivation; otherwise, instead of a few scanty traces of the disease in question, our early history and traditions would be full of its recollections, as they are of the malignant angina and the variola, and even our more recent writers must have had frequent occasion to allude to it, by the side of their typhoid pneumonia and petechial fever. Nor can I believe that the banks of the Kennebeck or the Merrimack, or even the Connecticut, have been so much more thoroughly drained and furrowed than those of the Hudson or the Genesee, as to account for their almost entire exemption from the endemics of the latter, and certainly the Seine and the Rhone, infested as they are with intermittents, had been for ages under cultivation, before the axe of the settler let in the light upon the rivers of the West.

Having glanced at these more obvious supposed causes, and leaving aside all discussion of the more subtle questions which naturally arise, but which are foreign to my present purpose, I come to the objects of my research, which I find

Quenched in a boggy syrtis, neither sea
Nor good dry land—

And which I must follow, like the fiend,

O'er bog, or steep, through strait, rough, dense, or rare—
And swim, or sink, or wade, or creep, or fly.

TESTIMONY OF THE EARLIER WRITERS ON NEW ENGLAND.

THE general accounts of the climate which they have transmitted, are, for the most part, very much in favor of its salubrity. In "A brief Relation of the Discovery and Plantation of New England," published as early as 1622, and republished in the Massachusetts Historical Collections,¹ the following account is given.

"But this country what by the general and particular situation, is so temperate, as it seemeth to hold the golden mean, and indeed is most agreeable to the nature of our own, which is made manifest by experience, the most infallible proof of all assertions; insomuch as our people that are settled there enjoy their life and health much more happily than in other places; which can be imputed to no other cause, than the temperature of the climate," &c.

In a paper entitled "New England's Plantation," &c. "Written in the year 1629, by Mr. Higgeson, a Reverend Divine, now there resident,"² is the following pleasant description. "The temper of the aire of New England is one special thing that commends this place. Experience doth manifest that there is hardly a more healthfull place to be found in the world that agreeth better with our English bodyes."—"And therefore I thinke it is a wise course for all cold com-

¹ 2d series, Vol. IX, p. 1.

² Mass. Hist. Collections, 1st series, Vol. I, p. 117.

plections to come to take physick in New England ; for a sup of New England's aire is better then a whole draught of Old England's ale."

In the same volume, at the 124th page, is "A letter sent from New England by Master Graves, Engynere, now there resident," in which are these expressions ; "—all which are made good and pleasant through this maine blessing of God, the healthfulnesse of the countrie, which far exceedeth all parts that ever I have been in. It is observed that few or none doe here fal sicke, unless of the scurvy that they bring from aboard the ship with them, whereof I have cured some of my companions onely by labour." And "Captaine Iohne Smith, sometimes Governour of Virginia, and Admirall of New England," who being the "pink of chivalry" was doubtless to be depended upon in his assertions, tells us, under the date of 1624, that the town of New Plimoth is "so healthful that of the first planters not one hath died this three years"—attributing the well known mortality among them at their first landing, to their "wandering up and downe, in frost and snow, wind and raine, among the woods, cricks, and swamps."¹

The statement of Wood, the author of "New England's Prospect," who left New England in 1633,² is more particular. "In *New England* both men and women keep their ordinary complexions, insomuch as seamen wonder, when they arrive in those parts, to

¹ Advertisements for the Unexperienced Planters of New England or any where, London, 1631. Reprinted in Mass. Hist. Coll. 3d series, Vol. III. Vide p. 27.

² Vide New England's Prospect, Boston, 1764, p. 51.

see their countrymen so fresh and ruddy. If the sun doth tan any, yet the winter's cold restores them to their former complexion; and as it is for the outward complexion, so it is for the inward constitution; not very many being troubled with inflammations, or such diseases as are increased by too much heat: and whereas I say not very many, yet dare I not exclude any, for death being certain to all, in all nations, there must be something tending to death of like certainty. The soundest bodies are mortal and subject to change, therefore fall into diseases and from diseases to death. Now the two chief messengers of mortality be *fevers* and *callentures*; but they be easily helped if taken in time, and as easily prevented of any that will not prove a meer fool to his body. For the common diseases of *England*, they be strangers to the *English* now in that land. To my knowledge I never knew any that had the pox, measles, green-sickness, headaches, stone, or consumptions, &c. Many that come infirm out of *England* retain their old grievances still; and some that were long troubled with lingering diseases, as coughs of the lungs, consumptions, &c., have been restored by that medicinal climate to their former strength and health. God hath been pleased so to bless men in the health of their bodies, that I dare confidently say it, out of that town from whence I came, in three years and a half, there died but three, one of which was crazed before he came into the land; the other two were children born at one birth before their time, the mother being accidentally hurt.”¹

¹ Op. cit. p. 11.

The author of the "Wonderworking Providence," in speaking of the year 1650, says that "This was the first noted year wherein any store of people died, *the ayr and place being very healthy naturally*, made this correction of the Lord seem the greater, for the most that died were children, and that of an unwonted disease here, though frequent in other places," &c.¹

In Hubbard's General History of New England² is still further confirmation of the fact so generally agreed upon, of the healthfulness of the climate. "Both the seacoast and the continent are indifferently mixed of mountainous champaigne lands, the aire thereby becoming more salubrious by far, than the next adjoining province of Virginia to the south, which consisteth generally both of a lower and richer soyle; it being found by experience that the vapours drawne out of the earth in the level and moister parts thereof by the directer beames of the sun, and not purified by the ventilating of the aire, as is usually seen in the higher and more hilly countrys, it useth to make the places more unwholsom and obnoxious to diseases, which the more hilly countrys are freed from."—"The heat in the summer and cold in the winter seldome are observed to continue in the same degree, but are very subject to suddoine alterations, from whence many epidemicall distempers are knowne to proceed oft times."—"However, the purity of the aire makes amends for the sharpness of the cold, being much cleansed in its lower rooms or chambers,

¹ Wonderworking Providence of Zion's Saviour in New England. London, 1654. Reprinted in *Mass. Hist. Coll. 2d series*, Vol. II, etc. Vide Vol. VIII. p. 19.

² Boston, 1815, pp. 19, et seq.

which are thoroughly purged thereby ; and so is the climate preserved from those rotting diseases of coughs and consumptions, which other countries where heat and moisture prevail, are more incident unto.”—And a little farther on, he concludes that “*the salubrity of the air in this country depends much upon the winter’s frost.*”

And not to omit the worthy old Governor Winthrop, we read in his Journal that “some families have forsaken both Providence and others the Caribbee Islands to come live here. And though our people saw what meagre, unhealthful countenances they brought hither, and how fat and well liking they soon became,”—etc.

The extraordinary pestilence which wasted the Indians some years before the arrival of the New England colonists, is mentioned by many of the early writers, but in general terms. The nearest approach to a description of this sickness, that I can find, is in the Collections of Gookin. He says, “What this disease was, that so generally and mortally swept away, not only these, but other Indians, their neighbours, I cannot well learn. Doubtless it was some pestilential disease. I have discoursed with some old Indians that were then youths ; who say, that the bodies all over were exceeding yellow, describing it by a yellow garment they showed me, both before they died and afterwards.”¹ It is worthy of remark, that Wood speaks of much underwood and certain rivers in some places where the Indians died of the plague (referring to the same epidemic) ; as between Wessagusset and Plymouth.²

¹ Hist. Coll. 1st series, Vol. I, p. 148. ² N. England’s Prospect, p. 18.

I will only add, with regard to this subject, that Smith declares that "what disease it was, the salvages knew not till the *English* told them, never having seene nor heard of the like before."¹

Although, as I have said, the early writers are very unsatisfactory in their accounts of the prevailing diseases, yet they have not failed to notice many of the more remarkable and fatal ones. But among all the writers I have examined, who allude to the diseases of the Indians and the Colonists, of whom I may mention Mourt, Winslow, Smith, Graves, Dudley, Winthrop, Wood, Morton, Johnson, Mather, Gookin, Josselyn, Hubbard, Callender, and Prince, the name of fever and ague, or intermitting fever, is mentioned by only four. We shall soon see how far their evidence is to the point. It is to be remembered, that however deficient their accounts and descriptions may be, they did notice the actual existence among themselves or the natives of many diseases not more extraordinary than the one in question. Thus we find, that in the authors cited, mention is made of scurvy, plague (a term applied to the Indian pestilence), small pox, fevers, malignant, infectious, and pestilential, fluxes, influenza, throat distemper, cholera morbus, or some disease resembling it, consumption, venereal disease, toothache, &c.; so that any considerable prevalence of intermittent fevers, appearing as they do in a particular district and season, could hardly fail to have been noted. I have stated this negative result without citing the pages where these facts are mentioned, or relating the circumstan-

¹ Advertisements, &c. p. 16.

ces given by the authors, although it would be only copying the abstract which I have prepared from the originals, and which is before me. (And I will add, because the Committee know that indexes are sometimes imperfect, that I have looked over all the works I mention page by page, with the exception of some few ecclesiastical papers, sermons, and similar treatises of Cotton Mather, which being more likely to cause a fever than to mention one, I left to some future investigator).

The first author who alludes to the disease, uses the following words. "Although the Indians be of lusty and healthful bodies, not experimentally knowing the catalogue of those health-wasting diseases which are incident to other countries, as fevers, pleurisies, callentures, *agues*, obstructions, consumptions, subfumigations, convulsions, apoplexies, dropsies, gouts, stones, tooth-aches, pox, measles or the like; but spin out the thread of their days to a fair length, numbering threescore, fourscore, some a hundred years, before the worlds universal summoner cite them to the craving grave,"¹ &c.

Having given the text and context, I proceed to my commentary. It is most probable that by *agues* the author means intermittent fevers, for the word is frequently used in this sense by the older writers, and never, to my knowledge, applied by them to those defluxions, as they would have called them, about the teeth and face, sometimes designated by the term among our own people.

But this loose testimony is evidently of no value,

¹ New England's Prospect, p. 114.

the assertion being coupled with others entirely erroneous. Thus it is said by Gookin, a much more competent witness, that consumption is common among the Indians,¹ and such is the experience of more recent times. And with regard to another affection, "Mr. R. Williams says, that when he first came here, the *Indians* were vastly subject to the Tooth-Ach, and that their very stoutest Men *complained more of that pain*, than their *Women* of the *Pains of Travail*."² The sprightly and poetical author of New England's Prospect seems to have taken a great fancy to the Colony, which he left, however, in 1633; and his descriptions are as florid as those of a speculator in Eastern lands talking of timber grounds on the Kennebec.

To balance the authority of this worthy adventurer, I shall now adduce that of "John Josselyn, Gent." of whom Mr. Savage says, in the notes to Winthrop's Journal, "His book is a curiosity, sometimes worth examining, but seldom to be implicitly relied on," and whose narrations are so liberally embroidered, that I cannot wonder at his finding some of them, as he says, "taken by some of my *sceptique* Readers to be monstrous lyes." He tells us that the diseases of the Indians are "pestilent Fevers, Plague, Black pox, Consumption of the lungs, Falling sickness, King's evil, and a Disease called by the *Spaniards* the Plague in the back, with us *Empyema*." "The Diseases that the *English* are afflicted with are the same that they have in *England*, with some proper to *New England*, griping of the belly (accompanied with *Feaver*

¹ Hist. Coll. 1st series, Vol. I, p. 173.

² Callender's Hist. Discourse, p. 101.

and *Ague*, which turns to the bloody flux, a common disease in the Country), &c.”

“September the 1 (1671), being Saturday, in the morning before day, we set sail and came to *Boston* about three of the clock in the afternoon, where I found the Inhabitants exceedingly afflicted with griping of the guts, and *Feaver*, and *Ague*, and bloody flux.”¹

The next citation which I shall make, is from Hubbard's General History of New England. This is the most perfect and satisfactory evidence I have been able to find among the early writers, and I need make no apology for giving it in full. It is to be found in the chapter headed “The first planting of New Haven. Some of the most remarkable passages concerning that colony,” &c.—“They have been at several seasons sorely afflicted with diseases, especially fevers, which have proved mortal to many. All that southerly part of the seacoast having, as more propinquity to Virginia in situation, so a participation with it in its climatical diseases, commonly there called the seasoning, which is an *ague and fever* seizing upon men in the heat of the summer, chiefly upon new comers, therefore called by that name, but not sparing the more settled inhabitants, especially in case of intemperate drinking. Upon these southern coasts of New England it is not annual, as in Virginia, there being sundry years when there is nothing considerable of it, nor ordinarily so violent and universal; yet at some times it falls very hard upon the inhabitants, not without strange varieties of the dispensa-

¹ Account of Two Voyages, &c. pp. 299, 333, 353.

tions of Providence, for some years it hath been almost universal upon the plantations, yet little mortality ; at other times it hath been very mortal in a plantation or two, when others, that have had as many sick, have scarcely made one grave ; it hath been known also in some years that some one plantation hath been singled out and visited after a sore manner, when others have been healthy round about ; so that the considerate inhabitants have seen cause to conclude, that though there might be something in the climate, yet a Divine Hand hath overruled, that so suitable acknowledgments of his greatness and sovereignty might be drawn from those that are unwilling to learn lessons of that importance. At one time or other every plantation, within less than these forty years, hath had its turn of heavy mortality, and some twice or thrice over ; and though somewhat hath been thought to be in the situation of the plantations, that some of them have not been so well seated for brisk and wholesome air, either for want of judgment in the planters, or overlooking that in comparison of other inconveniences, yet therein (not denying the ordinary interest of second causes) things have been carried above such sentiments ; while some plantations reputed most healthy have been turned as it were into graves, and others reputed for sickly have had a long and pleasant vacation. This disease, wherever it comes, is attended with great prostration of spirits, and sometimes in the hot fit with strange stupefaction of the brain. Strengthening the body with cordials, and gentle conductitious aiding of nature, hath been found better than sudden and violent means by purgation or otherwise ; and blood letting,

though much used in Europe for fevers, especially in the hotter countries, is found deadly in this fever, even almost without escaping; the reason whereof is left to be inquired by those it may properly concern. Setting aside the effects of this disease, those places have been generally very healthy, and, that notwithstanding have been all along, and are to this day, in a very increasing way, growing numerous, overstocked, and ready to look out for new plantations almost every where."¹

This statement, from so respectable an authority as Hubbard, would be amply sufficient to prove the former existence of intermittents, and that to a considerable extent, in at least one corner of the scarce-suspected soil of New England. We shall bring forward hereafter other evidence relating to the same locality.

The last positive mention of the disease which I have found in the works of the earlier writers, may be seen among the lives of those worthies whom Cotton Mather has immortalized in the *Magnalia*. The facts relate to Mr. John Sherman, Minister of the Church in Watertown; the famous John Eliot, commonly called the Apostle of the Indians, and Urian Oakes, sometime President of Harvard College.

Mr. John Sherman was born in Dedham, in the county of Essex, England, in the year 1613. He came to New England in the year 1634, and having remained a few weeks at Watertown as the assistant of Mr. Philips, he removed to New Haven. When the wind-colic, to use Cotton Mather's metaphor,

¹ Pp. 324-25.

carried Mr. Philips into the haven of eternal rest (which was in the year 1644), Mr. Sherman was called to succeed him. Here he appeared to have remained, and it is mentioned that he lectured for the most part once a fortnight in the neighborhood for more than thirty years together.

Of him our author says. "The last sermon which he ever preached was at *Sudbury*." "Being thus at *Sudbury*, he was taken sick of an Intermitting, but malignant *Fever*; which yet abated, that he found opportunity to return unto his own House at Watertown. But this *Fever* then renewing upon him, it prevailed so far, that he soon expired his holy soul; which he did with expressions of abundant *Faith*, *Joy*, and *Resignation*, on a *Saturday* evening, entering on his *Eternal Sabbath*, *August* 8, 1685. Aged Seventy two."¹

Mr. Eliot was born in England about the year 1605, and came to New England in the year 1631. Soon after his arrival he joined the church in Boston, and the year after was chosen as teacher and ruler of the church in Roxbury. In the words of Mather, "t was in the orb of that church that we had him as a *Star fixed* for very near threescore years." He visited other parts of New England in the course of his labors among the Indians, and among them Natick, Lynn, Mashippaug in Plymouth Colony, and Martha's Vineyard are mentioned. He commenced preaching to the natives in the year 1646. The circumstance which leads me to give these particulars, is the following, the date of which is not given, but which

¹ *Magnalia Christi Americana*. London, 1702, p. 165.

seems to have taken place in his later years, to judge by the first sentence.

"Had our Blessed *Jesus* at any time sent his wag-gons to fetch this old *Jacob* away, he would have gone without the least Reluctancies. Labouring once under a *Fever* and *Ague*, a Visitant asked him, *How he did?* And he reply'd, *Very well, but anon I expect a Paroxism.* Said the Visitant, Sir, *fear not*; but unto that he answered, *Fear! No, no; I been't afraid, I thank God, I been't afraid to die!*"¹

That Mr. Eliot exposed himself much in travelling, appears from his own words in a letter to Governor Winslow.

"I have not been dry Night nor Day, from the third Day of the Week unto the sixth, but so traveled, and at Night pull off my Boots, wring my Stockings, and on with them again, and so continue."²

Of his final sickness we only know that "He fell into some Languishments attended with a *Fever*, which in a few days brought him into the *Pangs* of Death."

Urian Oakes was born in England in the year 1631, and brought over to New England in his childhood. After finishing his education at Harvard College, he returned to England and was settled at Titchfield. At the death of Mr. Mitchel, which happened in 1668, Mr. Oakes was sent for to England, and came over to take his place as pastor of the church in Cambridge. I return to the words of my author. "The *Church of Cambridge* could now show

¹ *Magnalia Christi Americana.* London, 1702, p. 196.

² *Ibid.* p. 192.

this *Orient Jewel* for divers years, before the Almighty would have it made up *Among his jewels*; though the Troubles and Sorrows of a *Quartan Ague*, often diverted him from his Publick Services.”—“But the *Colledge* in *Cambridge* languishing under somewhat worse than an *Ague*, by the Want of a *Præsident* this Accomplished Man was invited unto that Place.”—“Soon after he had accepted his *Presidentship*, he was arrested with a malignant *Fever*, which presently put an End unto his Days in this World.”¹ (In the year 1681).

A few remarks with regard to these cases, may give a better idea of their value as evidence upon our question.

First, it is worthy of notice that Mather does not speak of intermitting fever, or fever and ague, or the quartan ague, as if there were any thing unusual in the affection which he designates by these different terms. Secondly, although the disease of Mr. Sherman is called an intermitting fever, the term commonly used by the older English writers, as Sydenham and Mead, for the disease in question, yet this single term is not as satisfactory proof of the nature of the disease, as the fact of Mr. Eliot's expecting a paroxysm at a moment when he was very well, or the mention of the quartan type, one of the rarer and more singular forms of the malady, in the case of Mr. Oakes. If this affection were really fever and ague, there can be little doubt of its having been indigenous in New England, for Mr. Sherman had been settled at Watertown for forty years, and in New England for fifty-

¹ *Magnalia Americana*. London, 1702, Book IV, p. 187.

one years ; and I may add, that if he brought his ague from England with him half a century before his death, he must have had a singularly happy constitution, to carry about his disease, to discharge his official duties, and to add twenty-six children to the growing colony.

There can be no doubt as to the fact of Mr. Eliot's being affected with fever and ague, and, it appears to me, very little that the disease was contracted in New England. As he came over at the age of twenty-six, and as the circumstance I have quoted happened late in his long life (if we may judge from the connection in which it is told), a regular and anticipated paroxysm could hardly have been due to any transatlantic exposure,—and certainly if the breath of Malaria hovered over a single swamp of New England, I know of none who was more likely to inhale it than the Indian Apostle.

There is no doubt, too, that Mr. Oakes had a fever and ague, but whether he brought it from England with him, or contracted it after his arrival, sleeps forgotten beneath the "*Hic jacet Oakesius.*"

This closes the testimony which I have found in the earlier authors. So far, then, as the question proposed can be answered from those records of the first century of New England which I have examined, it would seem that indigenous intermittent fever can have prevailed but to a very limited extent, and the only place which we can clearly point to, as giving origin to the disease, is New Haven.

EVIDENCE DERIVED FROM MORE RECENT SOURCES.

THE facts, negative and positive, which will now be brought forward, have been obtained principally from different medical journals and works published in this country, from the local histories preserved for the most part in the Collections of the Massachusetts Historical Society, and from the letters with which I have been favored from many of the most respectable physicians in New England. I may say, in this place, that in the course of these labors, nothing has given me greater pleasure than the general courtesy and kindness with which my requests for information have been received ; and that the strongest reason I acknowledge for desiring the success of this essay, is that the sacrifice of time and patience, which I have perhaps too freely asked of others, may not be rendered unavailing by any incapacity of my own.

As the starting-point of our medical survey is to be assumed at pleasure, I shall begin with the southwestern shore of Connecticut, the farthest corner of New England washed by the waters of the Sound. At this point, so close to the banks of the Hudson, and the city of New York, near which intermittents are well known, we might expect to meet with the same disease. In the place of proof I have nothing to give but probability. Dr. Clark Sanford, of Greenwich, Connecticut, one of the towns bordering on New York, in his "Observations on Peruvian Bark,"¹

¹ Medical Repository, Hex. 3d, Vol. III.

addressed to Dr. Rush, of Philadelphia, has the following words :

“The diseases which have occurred during the late summer and autumn, have furnished considerable opportunity for further use of the essential salt of bark. The effects observed in the treatment of *intermitting fevers*, correspond nearly with the opinion given of this preparation in my printed letter.”

As Dr. Sanford speaks of himself as a country practitioner, it is to be supposed that if he saw the cases to which he alludes, it was in his own vicinity, and very probably within the limits of New England. Such would, on the whole, be my inference from the sentences quoted, but it is possible that “the effects observed in the treatment of intermitting fevers,” were observed by others and communicated to him.

I have no record of the medical history of any other town along the coast until we come to New Haven. I have already adduced the testimony of one of the earlier writers to show that this town was liable to intermittent fever. In consequence of the circumstances mentioned, I addressed a letter to Professor Hubbard, of New Haven, who was kind enough to answer me in the letter from which I quote the following passages :

“I would observe that previous to the last seven years, I practised my profession in the town of Pomfret, Windham county, Connecticut, and of course as I was not able to answer the inquiries as it respected New Haven, I addressed a note to Dr. Eli Ives of this place, Professor of Medicine in the Medical Institution of Yale College. Dr. Ives has practised his profession in this place for more than thirty years,

and in addition to that circumstance, his father was a physician in this place, and he enjoyed the acquaintance and friendship of his elder brethren in the profession."

"In answer to my note, Dr. Ives remarks, 'Intermittents have prevailed in New Haven and its vicinity from time immemorial, as I learn from one who practised medicine for more than seventy years.' [Here Dr. Ives refers to the late Dr. Eneas Munson, who died in this place in the year 1826, aged 92 years]. 'A few cases' (Dr. I. remarks), 'have occurred since the year 1800, but I have not seen a case originating in this vicinity for twenty-five years.'"

Such is the information respecting New Haven, contained in Dr. Hubbard's letter, to which I shall again have occasion to refer, in speaking of some other localities which it mentions.

In a letter from Dr. Eneas Munson, Senior, published in Webster's Collection of Papers on Bilious Fevers, are the following words. "We have never, in this part of the country (except in a single instance, in the year 1743, mentioned in the preceding letter), been acquainted with a fever altogether similar to the one in question." "We have often seen continual endemial fevers, *intermittent*, and remittent bilious fevers, originating from the putrid gases of animals and vegetables, commixed, as from draining of ponds and stagnant waters,"¹ &c.

In a notice of the town of Guilford, in the Historical Collections, nothing is said of its diseases, and we therefore pass from New Haven to New London. In

¹ Page 191.

the 2d volume of the first Hexade of the Medical Repository, is a letter of the Rev. H. Channing, on the pestilential disease which prevailed at New London, in the year 1798, in which it is asserted that the town has "an elevated situation, with scarcely any low lands to generate marsh miasma," and "that it has ever been famed for the purity of its air and the health of its inhabitants." Nor, in the midst of the controversy which arose on the question whether the yellow fever of New London were indigenous or imported, have I found any thing to show that intermittents were known as an endemic in this place; which would seem most likely to happen, had such an endemic been known to the writers.

The next locality respecting which I have any information, is Block Island, which being an appendage of one of the New England States, I introduce in this connection. Dr. Willey has given an account in the Medical Repository of a fever which prevailed during the year 1801 in this island, and a medical history of the place in the same volume.¹ I will only say, that it appears from these papers that the diseases of the island had been little observed before his own residence upon it, and that there is no trace of the existence of intermittents to be found in these documents.

Returning to the main land, I find the following sentence in a description of South Kingston, R. I., by Dr. Joseph Comstock, dated 1811.² "Narragansett is a very healthful tract of country. Even those who live in the vicinity of this swamp have hitherto escaped *intermittents*, or any prevailing sickness. There is generally neither epidemic nor endemic dis-

¹ Hexade 1st, Vol. VI, pp. 123, 381.

² Hexade 3d, Vol. II, p. 430.

order amongst the inhabitants, ebriety among the lower classes excepted."

A letter, with which I have been honored from the venerable Dr. Hurd, of Concord, contains the following interesting statement:

"In the year 1777, the regiment commanded by Col. John Robinson, of which I was the surgeon, was a part of the time, say about the months of October and November, stationed at Quidnesset Point, lying about the south-east corner of South Kingston, Rhode Island.

"In October, a mess, so called, encamped in a low marshy place, about a mile from the regiment. Of those eight, three were taken sick. I visited them and found them attacked with the intermitting fever, correctly. I immediately ordered them all to be removed. In reply to your particular request, respecting the treatment, which I should think vain, had not you requested; I commenced with four grains of tart. antimony and five grains of submuriate. The operation was powerful with each. At bed-time one grain of opium. The days following, order pulv. cinchonæ freely. In four weeks they were on duty; no other case occurred in camp, nor in the neighboring towns.

"At the close of the year 1777, the regiment being disbanded, I returned to Concord, till 3d of February, 1778, at which time I commenced my profession in the town of Billerica.

"In the month of June I visited three cases at Pollard's tavern in Billerica, attended with the intermitting fever; but they were soldiers from the camp, and were transported to Reading. I left Billerica November, 1789.

"While at Billerica and Concord I have been repeatedly called on, in consultation, in several of the towns as far as Amherst, N. H., but never viewed the intermittent fever as indigenous to either of the towns. While I resided in Groton, under the tuition of General O. Prescott, there were two or three soldiers at Child's tavern, on return from camp, with the intermittent fever. I presume those cases occasioned the observations referred to in your letter."

As Providence is situated at the head of one of the larger bays of the New England Atlantic shore, I have thought expedient to introduce the facts relating to it in this place.

Dr. Wheaton, in his Topographical Sketch of this town,¹ remarks that "Intermittents and bilious remittents, so common in the Western and Southern States, have been unknown here for the last fifty years." This assertion would seem to imply that they were not unknown in former years, and such appears in point of fact to be the case. I owe the following statement to the politeness of Usher Parsons, M. D., of Providence.

"About eighty-eight years since, there was a dam thrown across Moochassuck River at the north end of Providence, for the purpose of establishing a water power. The pond overflowed a meadow, and as the water subsided, and brought the decomposing vegetables to a summer's sun, an intermittent fever attacked the inhabitants in the neighborhood very generally. It was of the tertian type, I infer from the fact that it was called the *third day fever*. Mr. Thurber, who

¹ Med. Rep. 2d Hex. Vol. IV, p. 333.

has resided for eighty years in the neighborhood, had his information respecting it from the mouth of some of the sufferers, and many others are familiar with the fact from tradition. Fever has in some instances been referred to this stream more recently, and to other ponds and rivers in this State, but it has always, within the remembrance of living witnesses, been of the bilious remittent or simple continued type."

Dr. Parsons adds, also, "I am informed by Dr. Hudson, now seventy-five years of age, living in Cranston, seven miles west of Providence, that there occurred in his neighborhood, between forty and fifty years ago, three cases of intermittent fever, in patients residing within a few miles of each other; that one of them was of the quotidian or daily type, and the others tertian; that they all yielded to treatment with bark or Fowler's solution, though not until they had several paroxysms; that neither of them had ever been abroad previously, rarely out of town, and probably never out of the county. They were all females. He has never known any other cases since. His aged lady remembers them perfectly well, even to the employment of Fowler's solution, which was then recently brought into use."

If we now return to the mouth of Narragansett Bay, we find on the eastern side the town of Compton, or Little Compton, as it is designated in the map, of which we have some notice in the Historical Collections.¹ In this account, it is said that the most common disorders are inflammatory, that pleurisies some-

¹ 1st series, Vol. IX, p. 199.

times prevail in spring, but that, on the whole, it is remarkable for the health and longevity of its inhabitants. This is the kind of negative evidence to which we must frequently appeal for want of better demonstration. It is pretty good proof that so striking an endemic as fever and ague has not been known, to the narrator; and in country towns, where traditions are so frequent and so familiar to the intelligent inhabitants, it gives a considerable probability that no distinct traces of the disease are perpetuated in the memory of their patriarchs and oracles.

Passing along the coast to New Bedford in Massachusetts, I can only cite the authority of a memoir in the same Collections referred to,¹ in which it is said that "the air of the place is salubrious, and there are instances of longevity."

In the accounts of Rochester, in the same publication,² a mortal fever is spoken of as existing in 1816, which spread from Fairhaven, and had occasioned the death of fifty inhabitants. They are asserted to be generally long-lived.

In the Addenda to articles on towns in the county of Plymouth (Hist. Coll. 2d series, IV, 302), are these remarks. "It is stated, as a fact, that this epidemic³ followed the course of rivers, tracing up the Accushnet and Mattapoiset, to the great pond in Freetown, and not extending but very little beyond the meeting-house in Rochester, which has ever been one of the most healthy spots in New England, and where it is dry and sandy. Dr. Mann further states,

¹ 1st series, Vol. IV, p. 232.

² 2d series, Vol. IV, and Vol. VIII.

³ That of 1815-16.

we are informed that scarce a person escaped this fever, who lived within a mile of the great pond in Sharon, where it also mortally prevailed. Six persons, of the family of Ashley, died in one house of this fever, situate near the great pond in Freetown. This singular disease seems therefore to have a choice of location, humid and swampy situations."

In an account of Wareham, in the fourth volume of the second series, this town and its vicinity are pronounced healthy, upon the authority of Dr. McKee, long a resident physician in the place. In the description of Dukes county,¹ it is said that fogs are frequent, but not unwholesome; that consumption is the most fatal disease; that fevers, dysentery, &c. are not common. In Chilmark, one of the towns on the island constituting this county, of one hundred and thirty-nine deaths, which took place in the course of nineteen years, five are said to have been caused by dysentery, five by bilious fever, and two by yellow fever. It is also said that the Indian plague did not probably visit Martha's Vineyard, but that there was, in 1643, a severe disease, probably yellow fever. These details are mentioned as giving value to the absence of any mention of intermittent fever.

With regard to the island of Nantucket,² it is asserted that its diseases are not very different from those of the main; that pulmonary consumption is not as common, that the place is for the most part healthy; and that an uncommonly mortal distemper prevailed among the Indians in 1763; but there is no mention of fever and ague either in the course of the description, or in the bill of mortality given.

¹ 1st series, Vol. III.

1st series, III, 153. 2d series, III, 19.

Many of the towns in Barnstable county are mentioned in the first series of the Historical Collections, but in none of these accounts have I found any allusion to intermittent fever. Such facts as relate to their general salubrity, and their diseases, I will briefly exhibit in the geographical succession of the places named.

In the accounts of Falmouth and Yarmouth there is no mention of their diseases.

Chatham.¹ Fogs very frequent. Air considered salubrious. There is not enough employment for a physician.

Eastham.² The climate is not materially different from that of other parts of the county. Hysterical complaints, nervous disorders, and consumption are common.

Wellfleet.³ In 1772 a fever prevailed, which occasioned forty or fifty deaths. Since that, it has been healthy as other places. Sore mouth and sore throat are spoken of as common.

Truro.⁴ Climate favorable to health and longevity. Nervous complaints very common.

Provincetown.⁵ The climate and diseases do not differ materially from those of other parts of the county. In 1794, a fever, supposed to be caused by putrefying sharks, proved very mortal.

Dennis.⁶ Climate healthful; bilious and nervous disorders, rheumatism and consumption the most common diseases.

Barnstable.⁷ Air damp and chill; nervous complaints and consumption common; but place not unhealthy.

¹ Vol. VIII.

⁶ Ibid.

² Ibid.

⁷ Ibid.

³ Vol. III.

⁴ Ibid.

⁵ Vol. VIII.

Brewster.¹ Climate seems conducive to health. Number of annual deaths, small.

Sandwich.² Climate favorable to longevity.

I have no other evidence relating to the towns upon this part of the coast, before I arrive at Plymouth. In the third volume of the second series of the Historical Collections, is a memoir of this town, in which it is said, upon the authority of Dr. Thacher, that "The diseases most prevalent for the last ten years, are fevers of the typhoid type, but not remarkably malignant or mortal." I have been honored with a letter from this venerable physician, from which I make the following extracts :

"Indigenous intermittent fever has at one period occurred in the course of my practice. I made no minutes at the time, and must now rely on my recollection and that of Dr. Hayward, who was then my partner in practice. It was at a place called Eel River, three miles south of this town. The situation was a low wet swamp of considerable extent, and the soil is clayey for some distance around. No particular disease was ever known to prevail there, till about thirty years ago, when it became necessary to drain off the water from the swamp, for the purpose of constructing an iron factory ; this exposed to the influence of the sun an extensive surface of swampy muddy ground, which had always before been covered by water ; the consequence was, that two families, residing in the immediate influence of this newly created atmosphere were attacked by intermitting fever. The number of cases did not ex-

¹ Vol. X. ² Vol. VIII.

ceed four or five. Whether this was in the autumnal or vernal season, I am unable to determine. The type was, I think, a mild tertian, and all the cases yielded speedily to the common remedies. Excepting this solitary instance, I cannot learn that the disease has ever been known in any part of this vicinity, unless when brought from the southern States, and they were in general soon cured after their arrival here."

"I had forgotten to mention that the unhealthy spot is about one mile from the seashore, and the tide rises about half the distance, but very high tides reach near to the iron works; and since the works have been constructed, a large pond, created by a dam, covers all the swampy ground."

The disorders most prevalent in Kingston are said to be consumption and putrid fever. The dysentery was very mortal in 1776. The Indian pestilence swept off the natives, who had been very numerous.¹

Of Duxbury it is said that the air is friendly to health and longevity, that no ill effects are experienced from the east winds, and that the inhabitants are not more exposed to disease in April, May, or June, than in other months.²

Scituate is said to be generally healthful. "In a section of low lands, in the north parish, fevers annually prevail,"³ but of what type does not appear.

Cohasset is considered salubrious. No epidemic proving very mortal has been experienced for many years.

¹ Hist. Coll. 2d series, III, 204.

² Ibid. 2d series, Vol. III.

³ Ibid. IV, 228.

In the notices of Dorchester¹ and Brookline,² there is no mention of intermittents among the diseases spoken of.

Boston. In an elaborate account of this city in the Historical Collections,³ by the author of the Historical Journal of the American War, are these passages.

“As effluvia arising from marshy ground are allowed to be the largest source of putrid disorders, it is probable that the exhalations promoted by the heat of the sun acting on a large extent of flats and marsh, especially at the southern extremity of the town, may justly be considered as a very operative cause to produce putrid disorders.”

“No one disease can properly be said to be endemic.” “Consumptions and dysenteries, with putrid fever, may perhaps emphatically be styled the diseases of the place.”

In the bills of mortality for the city under consideration, many of which I have looked over in different periodical publications, a case of intermittent fever is occasionally to be seen, but the rarity of these instances is doubtless owing to their being only casual importations from other places. And, in reporting some cases in one of the journals, which were treated at the Massachusetts General Hospital, Dr. Channing, of Boston, speaks of them as if it were a matter of course, that they originated at a distance.

Dr. James Jackson has favored me with the following communication, detailing a case which has all the aspect of indigenous intermittent, and containing also his general testimony on the point in question.

¹ Hist. Coll. 2d series, Vol. IX.

² Ibid. Vol. II.

³ 2d series, III, 241.

"I was called, one day, a number of years since, about 1 o'clock, P. M., to consult with the late Dr. Dixwell respecting a lady under his care then urgently sick. She lived in Essex street, Boston, and had lived there for a year or two, and had not been travelling for some time previous, if for many years. She had passed most of her life, I believe, in Gloucester, Mass., but had resided in Boston two or three years or more. Dr. Dixwell stated that she had had what appeared to him to be an intermittent fever; the type, I remember, was tertian. I made every inquiry which the time would permit to ascertain if she had not some local disease, but found no evidence to support the belief that she had. I likewise made inquiries as to any cause which could be the source of the disease; none appeared, unless it could be the margin of the bay, covered twice a day with the tide. Dr. Dixwell gave an account of her paroxysms, stated that he had treated her with cinchona (in powder and tincture); he stated that the paroxysms had ceased, and that on this day he thought her perfectly secure from a recurrence of them. At or near 12 o'clock, she was dressed to go abroad, having felt perfectly well all the morning; but just when ready to go out she was attacked suddenly with severe chills and rigor. When I saw her, this chill continued with extraordinary severity, such as to affect the respiration, making it almost impossible for her to speak, attended by a coldness of the surface which could not be overcome, and by more or less lividity of the surface. There were other symptoms which I do not distinctly call to mind; I only know that there was none which seemed to me to prove the existence of any local dis-

ease, though I was most strongly disposed to think that such disease must exist. There was, indeed, a difficulty of respiration, but this seemed to belong to the muscles of the thorax, which were affected in common with the other muscles. She had a sense of great distress, not accompanied by any affection of the mind, nor by a failure of any of the functions connected with the brain. In about three hours from the seizure, as nearly as I can recollect, she died, under the phenomena of the cold stage. No autopsy was permitted.

“This is the only case I have ever known of intermittent fever in Boston, or the vicinity of Boston, so far as I am acquainted with that vicinity; nor have I heard on any good authority of a case of this disease in my day within these limits.” Dr. Jackson does not mean to give this as an unquestionable case of intermittent.

In the accounts of Charlestown, in the *Historical Collections*,¹ and the *Medical Repository*,² there is no allusion to intermittents. In the last paper (by the Rev. Dr. Morse) it is said, “the inhabitants are subject to no endemical diseases.”

In the Rev. Dr. Holmes’s account of Cambridge,³ the air of that town is said to be very pure, and many of the inhabitants are asserted to have obtained great longevity.

In the Rev. William Bentley’s *History of Salem*,⁴ no mention of intermittents is found among the fatal diseases enumerated. Dr. Jackson, of Boston, says, in the communication already referred to, “I have heard

¹ 2d series I, 165.

² *Hexade* 1st, Vol. II. p. 10.

³ *Hist. Coll.* 1st series, Vol. VII.

⁴ *Ibid.* Vol. VII.

Dr. Holyoke state that he had never known any case of intermittent originating in Salem, and he considered that the disease never originated in this section of country."

For want of information, I must pass over the intermediate towns between Salem and Newburyport. With regard to the latter town, I can only adduce the following statement which I owe also to Dr. Jackson, of Boston.

"When a boy under the age of fifteen, I heard the late Dr. Swett of Newburyport, speak of a poor family in the neighborhood, living on the margin of a small pond from which he was desirous that they should be removed, because they had intermittent fever. He spoke of this as if it had recurred at different seasons. Dr. Swett was a man of sound mind, to be relied upon in his diagnosis."

From a letter for which I am indebted to the kindness of Dr. Jabez Dow, of Dover, I have derived the following information respecting Kensington, N. H., the place of his nativity. "It has been my belief for more than thirty years," he premises, "that intermittent fever was once indigenous in New England, from a considerable portion of sound tradition, a few small records, and from seeing the disease indigenous two or three times in the same period, which has long caused me to wonder why this inquiry has not been made many years ago. It is perfectly within my recollection that the people then past middle age, now and then related instances of fever and ague that occasionally occurred in that town and in the towns around it, but had never seen any thing of it; nevertheless they often stated with the rest, that they had

often heard old people say when they were young, that fever and ague had occasionally occurred when the town was first settling and clearing for farms, which took place more than a century ago; also that it arose from some unknown cause among themselves, and that it gradually disappeared as the town became more and more settled, and more thoroughly cultivated. This information occurred to me, from the age of fifteen to twenty years."

Dr. Dow speaks also of a disease among children, which had some of the characters of remittent fever, by his description, and of cynanche and scarlatina which appeared in this town (Kensington) in the year 1801, the existence of which diseases, he mentions in connection with "exhalations from the meadow ground," whence it is to be inferred that Kensington has at least one of the common sources of Malaria.

Dr. Dow had the opportunity of inspecting the bills of mortality of Kensington, kept by the Rev. Mr. Fogg, and his successor, Rev. Mr. Shaw, and extending from the year 1737 to beyond the year 1800.

"In this bill of mortality was mentioned death by fever and ague, and intermittent fever, several times in the course of several months. This led to a further inquiry concerning this last disease, when the result came out much the same as it did some years before, only the story was more particular, and better understood probably by myself. The younger Mr. Fogg said, he always understood from his father, and the people of his early days that the disease occasionally made its appearance from the first settlement of the town; was a very mild disease compared with this disease at the South, and disappeared probably before

the middle of the last century ; he understood that the deaths by this disease happened in cases of aged people, persons of feeble constitutions, or an ineffectual treatment for want of knowledge, or no treatment at all ; that it was indigenous, can scarcely be doubted, as there was hardly a possibility of bringing it from any place out of New England, as their intercourse with such places was almost nothing at all. I took the liberty to omit the names of the diseased persons, and to make some changes in the names of the diseases, and then published it, and gave a number of them to the Medical Society."

In a description of the Isles of Shoals,¹ it is stated that, "the weather is very bleak here in winter, but it is delightfully cool and salubrious in summer, and at all seasons very healthful. Among all the exposures necessary to their occupation, the inhabitants have seldom need of a physician ; and no one of this profession has lived on the island for more than twenty years."

Bills of mortality for Portsmouth, N. H., by Lyman Spalding, M. D., may be found in the Collections just cited,² in the Medical Repository,³ and in the Medical and Agricultural Repository. The existence of bilious remittent and bilious malignant fever is spoken of in these papers, and in each of the years 1802, 1803, and 1807, there was one death from intermittent fever. Whether these cases were imported, I am unable to say, and I have not as yet received any private information from this place.

¹ Hist. Coll. 1st series, Vol. VII.

² 1st series, Vol. IX.

³ Hex. 2d, and Hex. 3d.

The Hon. David Sewall, Esq., says, in his memoir of the town of York,¹ "The climate is healthy, many living to between ninety and a hundred years; from computations for a series of years (thirty or forty past), one in six or seven of the deaths have been in persons upwards of seventy years of age."

It is stated in the account of Saco, that there are usually thirty deaths a year, and that no disease has prevailed.

Professor Barnes, of Waterville College, has taken the trouble to make some inquiries respecting the existence of intermittents in the State of Maine, and I extract the following paragraph from one of his letters.

"My last informant says, that his father clearly had the fever and ague about eighty years ago; and all the tradition shows, that it must have been indigenous. The old man often spoke of it to his last day, and the fact seems fully established. He lived in Biddeford, near the mouth of Saco river, say two and a half miles from the sea, and about as far from the river. The character of the immediate spot I do not know, but the whole of that part of the coast is easily described. Extensive granite ledges; a good deal of salt marsh, intersected by upland ridges, covered naturally with pine, and sometimes expanding into pine barrens. All that can be known now about the points of this case is, that *hot toddy* in any quantity made no impression upon the ague fit, and the previous vigor of the constitution was never afterwards recovered, debility of the joints being particularly obvious as an effect."

¹ Hist. Coll. 1st series, Vol. III.

In "an account of febrile diseases, as they appeared in Portland and its vicinity, in August and September, 1801,"¹ it is stated, that "in September malignant fevers occurred in different parts of the country, near stagnant ponds of water. In some few the skin was yellow, in others purple spots appeared." But neither in this paper nor in several others, by the same gentleman, on the diseases of Cumberland county, is there any mention of intermittents.

In the notices of Freeport and Georgetown, in the Historical Collections, there is no mention of the prevailing diseases.

Topsham is said, by the Rev. Jonathan Ellis, to have a healthy climate;² and the Rev. Alden Bradford makes the same assertion respecting Wiscasset, and the towns in Maine generally.³

Of Thomaston, it is said, that it is presumed, that for health and purity of air, no town in the State can exceed it.⁴ In the same article, the air on St. George's river is said to be healthy and pure, like that of most of the Eastern country.

Machias is the last town on the seaboard, of which I have any notice, and its diseases are not mentioned in the article referred to.

¹ Med. Rep. Hex. 1st, Vol. VI.

² Hist. Coll. 1st series, III, 141.

³ Ibid. VII, 176.

⁴ Ibid. IV, 20.

FACTS RELATING TO PLACES SITUATED UPON THE LARGER RIVERS,
AND THEIR TRIBUTARIES.

RETURNING once more to the western section of New England, the first considerable river which attracts our notice is the Housatonic.

This stream has been long known as the centre of a region more liable to intermittents than almost any other section of New England. I proceed to bring forward the evidence in proof of their indigenous origin in this situation.

As containing testimony of a general nature respecting this river, I offer the following letter of William Buel, M. D., of Litchfield, Connecticut. This is one of the two communications received by the Committee of the Massachusetts Medical Society in reply to their circular; and I am indebted to the kindness of Dr. Walker, of Charlestown, for the use I have been allowed to make of it. The date is June 6th, 1835.

Dr. Buel states that he has been employed "during a considerable part of a long professional life, in situations which, as he conceives, were adapted to develop facts having a bearing somewhat important on the objects contemplated by the counsellors of the Massachusetts Medical Society."

He says, "the phenomena from which the statements and remarks about to be submitted were principally derived, occurred in the valley of the Housatonic, a stream of two or three hundred yards in width, running near the western border of the States of Massachusetts and Connecticut, and emptying into

Long Island Sound. This stream, for thirty or forty miles above Canaan Falls, meanders through a valley of from one to five or six miles in width, of alluvial formation. Its course is serpentine, and from the circumstance of the region being nearly of a dead level, its current is ordinarily very sluggish. It is very liable to be so swollen by heavy rains as to overflow its banks and extensively inundate the adjacent flats. Such an inundation almost uniformly succeeds the thawing of the snow in the spring, and not unfrequently occurs at all times in the year. From the frequent occurrence of new channels, occasioned by the abrasion of its alluvial banks on one shore and deposits on the other, new channels are constantly forming, leaving beds of the old one isolated reservoirs of stagnant water charged with copious deposits of decaying vegetable substances floated into them by successive inundations, being thus rendered sources of permanent deleterious exhalation."

"Situations circumstanced like those above described, have been from time immemorial recognised as originating gaseous matter, having an agency in the production of intermittent fever and its kindred diseases. And although it is not cognizable to any of the senses, the existence of such an agent may be considered as indubitable, as where such a state of things is found to exist, there a specific class of diseases is developed; and where that, or something analogous does not exist, none of that class of diseases are to be found."——

"Mill-dams on the Housatonic, and its tributary streams, by forcing the water for miles above their location into low grounds, marshes and coves, and

thereby producing macerating reservoirs of vegetable substance, produce foci of pestiferous exhalations, to which intermittents, in all their grades and varieties have been obviously traceable."——

"I would add, in this place, that opportunities which I had during a part of one season, of being conversant with the phenomena of intermittent fever about the lake region in the west part of the State of New York, and in an early period of the settlement of that country, afforded, to my mind, conclusive evidence of the identity and simplicity of the laws of malarious disease, wherever circumstances exist adapted to occasion its development."

"Having made these preliminary remarks, I will now allude to the queries of the Committee in their order, and briefly submit in answer some facts which have occurred to me, of a nature to have a bearing on the subject of their inquiry."

"And first, that there are places where intermittent fever in former periods has existed as an endemic, and where it does not now, I consider myself warranted by experience in many instances in affirming. Places affording unequivocal and conclusive evidence of this fact, are those where the disease has been created by mill-dams. I have known several instances where dams have been erected on the Housatonic and its tributary streams, giving rise to the disease in their vicinity, and of its entire cessation on their removal. I have known instances where intermittents apparently resulted from the felling of large quantities of timber, permitted to go into a state of decomposition, ceasing entirely after the lapse of years. In the lake region, in the western part of

the State of New York, already alluded to, intermittent, for several years after the commencement of its settlement, was extensively prevalent where it is now nearly extinct. This revolution may be philosophically accounted for from the effects of general and highly advanced cultivation. As respects the valley of the Housatonic generally, I believe the prevalence of intermittent fever has for the last twenty or thirty years been gradually diminishing, and without doubt the sources of Malaria have been lessened by the extended cultivation of the soil, the draining of marshes, and in some instances the removal of mill-dams, and the cultivation of the ground formerly inundated. In the town of Litchfield, Conn., a small stream, called the Bantam, meanders for several miles through a tract of alluvial formation, expanding into ponds of several hundred acres in extent. In an early period of the settlement of the town, the inhabitants erected a dam on the stream, near where it emerges from the flat, for the purpose of killing the timber with which it was covered by inundation. It produced the intended effect; but another result was, as I was informed by one of my ancestors, who witnessed the transaction, that a general prevalence of intermittent fever affected the inhabitants of the vicinity. After the water was reduced to its natural channel, by the removal of the dam, the disease subsided, and the inhabitants have ever since, for the period of more than a century, remained exempt from all malarious affection.

“The statements already made may be considered as in some measure replying to queries 1st and 2d of the Committee. In reply to 3d and 4th,

I would remark, that intermittent fever still prevails to some limited extent in the valley of the Housatonic. In the town of Pittsfield, Mass., situated near the commencement of the stream, where the disease was formerly of very common prevalence, it has become nearly extinct, owing undoubtedly to the removal of mill-dams, the draining of marshes, and the state of cultivation to which that region has attained. In New Milford, Conn., a town in the same valley, a great diminution of malarious disease has also taken place, and from similar causes. Indeed the same causes, I apprehend, have operated more or less in lessening the frequency and mitigating the malignity of the disease throughout the valley.

“No place is within my knowledge where malarious disease has recently been produced *de novo*. Some six or eight years since, a dam was thrown across the Housatonic two or three miles south of the dividing line between Massachusetts and Connecticut, for the purpose of obtaining water power for mechanical purposes; the effect of which was, to raise the water in the stream, and to affect the adjoining flats and reservoirs of stagnant water along its course to the distance of ten miles above. An increase of intermittents was believed, by the inhabitants of the region affected, to ensue to such an extent that they were induced to seek and obtain redress from municipal authority, on the ground that the effects of the dam on the public health were such as to constitute it a public nuisance. I have no doubt but damming streams flowing through flat regions, under circumstances favoring the decomposition of vegetable substances, would in all cases generate malarious princi-

ple, and produce the same effects which have hitherto resulted from similar causes.

"No. 5. As to the type of intermittent fever, wherever I have been conversant with the disease, either in the lake region of New York, or in the valley of the Housatonic, it has generally been tertian, not unfrequently quotidian, rarely quartan. In answer to the part of No. 5, 'Was it connected with or did it pass into common or continued fever?' I would remark, that if any other fever be meant than that which has the distinguishing characteristics of malarious fever, that is, a strongly marked tendency to intermittent or remittent type, I should express a decided and unqualified negative. If intermittent fever be taken as a generic term for malarious disease, that affection, I believe, will be found in all instances exhibiting such decided tendency to intermittent or remittent type, as to distinguish it conclusively, not only from the common continued typhus or synochus of our country, but from all other febrile diseases. Its intermittent or remittent form is more or less slowly developed according to circumstances, more speedily and perfectly in vernal than in autumnal disease; more in temperate, regular and abstemious subjects than in inebriates, and those who have been accustomed to alcoholic stimulus. But the disease is unquestionably much modified in its type, character, and malignity, by epidemic influence. This agency, leaving the philosophy of its nature, cause and origin to be investigated and explained by those who are competent to do the important subject justice, I have found to be extensively operative in malarious disease, not only in retarding its tendency to regular intermittent type,

but in giving it at times great malignity of character. I witnessed the deleterious bearing of this principle on malarious disease, both in the lake region, New York, and on the Housatonic, in the years 1795 and 1796, to an extent that would be hardly credible, were its effects truly depicted. In addition to greatly diminished tendency to intermittent or remittent form, alarming cerebral affection, in the form of coma, delirium, subsultus, &c. ; red, dry, smooth, and shining tongue ; intensely yellow skin, and other symptoms of hepatic, and general visceral derangement ; insusceptibility to the effect of remedies, and even fatal termination, were not uncommon occurrences in those years, in places where the disease had previously prevailed with ordinary symptoms. And after such a course of suffering, incipient remission would hardly commence, before a recurrence of paroxysm would hurry on a renewed tempest of symptoms, which the strongest constitution seemed unable to sustain. This state of things would frequently continue for several days, before such an interval could be obtained as to afford an opportunity for the exhibition of remedies adapted to suspend the disease, especially in subjects who had not experienced acclimation. This highly exaggerated state of disease in certain seasons, cannot, as I conceive, be accounted for on any other principle than epidemic influence. Additional evidence of this influence, during the period to which I allude, may be derived from the fact of the unusual prevalence and malignity of character of yellow fever in our cities, of dysentery in many parts of our country. In the summer and autumn of 1796 a most devastating dysentery prevailed in Sheffield, on the

Housatonic, corresponding in time and limits very exactly with the malarious fever, and probably participating both in its endemic cause and epidemic influence. A similar endemic dysentery is described by Dr. Oliver Fisk, in Vol. II. of medical papers of the Massachusetts Medical Society, as prevailing in Worcester, Mass., at the same time as in Sheffield.

"The extensive agency of epidemic influence in originating disease in some instances, and modifying and aggravating it in others, is of sufficient notoriety, and however difficult of explanation, is of such paramount importance as to entitle it to important consideration in investigating the laws of human disease.

"To the first inquiry in No. 6, I would reply, that a reduction of the disease to an intermittent or remittent form, was considered as a primary and important consideration. This was best effected by the free use of cathartics, of which calomel was generally an adjunct, especially where the functions of the liver were morbidly implicated. In some cases emetics were used advantageously, venesection rarely. After due preparation, the free use of the Peruvian bark, and generally in substance, was resorted to, and viewed as a specific; not more than one or two paroxysms recurring, generally, after its free exhibition during one or two intervals. Its use was continued in lessened quantities after the suspension of paroxysms, with a view to fortify the system against a recurrence. As to the duration of malarious disease, I would remark, that whether vernal, and of mild and intermittent form, or autumnal and only remittent, or when malignant and hardly remittent, if not suspended by the bark or other specific remedies, a continuance

for weeks or even months might be expected. It is the nature of the malarious principle to cling with great obstinacy to the constitution in all the variety of fever which it occasions; and a disposition to recur, whether permitted to run its course or when suspended by remedies, is its decidedly characteristic trait. Autumnal disease, after having remained suspended during the winter, almost invariably returns the ensuing spring, and generally in a regular intermittent form. Relapses are at all times liable to be induced by fatigue, and all irregularities. In reply to the remaining queries of No. 6, I would remark, that in addition to the languor and debility consequent to other febrile disease, there was a peculiar sallow paleness of skin, owing to biliary tinge, and which may be considered as indicative of there having been peculiar determination of the disease to the hepatic functions, and jaundice is not unusually a consequence of malarious fever. A good constitution would ultimately, in most cases, react with sufficient vigor to attain a state of health. But the disease is occasionally followed by 'such maladies as are commonly described as following intermittent fever.'

"No. 7. There is not only probability, but certainty, as I conceive, that in many cases the subjects of intermittent fever have been exposed to its causes in some other place or part of the country, and that the malarious principle may remain dormant in the system over one winter, and to the extent of nearly twelve months, and then produce its specific morbid effects. I have known many cases unquestionably of this character."

I shall now relate such evidence as I have been able to find respecting the towns on the Housatonic, beginning at those nearest its mouth. I shall be obliged, however, in order to preserve in some measure the integrity of the communications I have received, to bring forward some of my evidence a little out of the natural order.

New Milford. The following account of a trial at law, by the Hon. David Dagget, may be found in the first volume and part of the Memoirs of the Connecticut Academy of the Arts and Sciences.¹ I omit certain parts not essential to my present purpose.

“Before the Superior Court, held at Litchfield, on the fourth Tuesday of January, 1800, was tried an action of trespass, instituted by Joseph Ruggles, of New Milford, against Elijah Boardman, and others, inhabitants of New Milford. The claim on the part of the plaintiff was, that the defendants, in January, 1799, destroyed a part of his mill-dam, erected across the Housatonic river, and nearly opposite the most compact part of the town. The defendants acknowledged that they had injured the dam, in manner as alleged, and justified, on the ground, that the dam was a public nuisance, in that it was the cause of a distressing sickness, which had for several years visited New Milford. It was agreed that a dam had stood at or near the place of the present dam, for about sixty years past; and that the dam complained of, had been by the plaintiff, in July and August, 1796, raised about ten inches. It was also agreed, that a bilious remitting fever, and the fever and ague, had

¹ Published at New Haven in 1810.

raged with great virulence, in the vicinity of this dam, in the years 1796-7-8 and 9. The great question, therefore, in the case, was, whether the raising of the dam in 1796 was the *sine qua non* of the disease? A variety of testimony was produced by the parties, tending to convince the court and jury of the truth of the affirmative and negative of this question. It was proved, that in each of the years above mentioned, an unusual sickness had prevailed; that the whole number afflicted with the bilious fever was about 300; that this fever commonly began in July, and ceased in October; that the fever and ague had also been prevalent in the period aforesaid, but was not confined to place or season. It was also proved, that there were upwards of fifty acres of low marshy ground, on the west side of the river opposite the town; that there was in July and August, much stagnant water in and about those marshes; and it was contended (though the fact was doubtful) that the waters in and about those sunken places, were materially affected by the raising of the dam. To prove that this state of the water, &c. might, and probably would produce the fever, the opinion of physicians, and the existence of similar facts in other places were resorted to.

“Of the physicians who had viewed this dam, and the mill-pond made thereby, with the circumstances and situation of the town, some were of the opinion that it was the cause of the sickness, while others doubted or disbelieved it. It was proved that the raising of the waters by mill-dams, in Salisbury, Colebrook, Roxbury, and in various places in the States of Massachusetts, Vermont, New York, and Pennsyl-

vania, had been followed with fevers, of the same type with that at New Milford. It was contended by the plaintiff, that raising the dam would not be injurious, unless thereby more ground was overflowed, from which effluvia would arise; and this was denied, since the water was now kept within the well-defined banks of the river;—that the situation of the town was favorable to disease, being circumscribed by high hills, and consequently subjected to a bad state of air; and that there were causes sufficient, without resorting to the dam, to account for the fever. It was proved, that in the year 1796, as early as the 20th of July, there were many cases of the bilious fever, strongly marked; and that, at that time, the dam was not raised or altered from its usual height;—that the same fever had existed in many preceding years, from 1782;—that in 1799, after the destruction of the dam complained of, and while it stood with the water at its ancient level, the same fever raged, though with less malignancy, and in situations more remote from the mill-pond. These were urged as sufficient to encounter the presumption arising from the facts previously stated.

“It was also proved, that in 1757, a malignant fever (as it was then denominated), raged, to the destruction of about forty inhabitants;—that in 1777, the dysentery prevailed, said to have been brought from the army, and the fever and ague had always been a disease of New Milford;—that the towns through which the Housatonic river runs, have been frequently visited with bilious fevers, and that too where no mill-dams could be resorted to as the causes.

“The physicians concurred in opinion, that persons are seldom attacked with this fever more than once during an epidemic, but that the fever and ague frequently visits the patient, in the spring or summer following. They also agreed, unanimously, that from 1793 or 94, fevers have been more frequent and malignant, than in any preceding years, excepting that in the last season there appeared an abatement in the number of cases, and violence of the disease.

“It was proved that the same disease with the one under consideration, had prevailed in many places, in this and the States of New York and Massachusetts, within the last five years, where no mill-dams or ponds could have operated—on the most elevated hills, and in situations heretofore deemed the most healthy; that in Great Barrington and West Stockbridge, the disease appeared remote from the ponds, while the people in the vicinity of them enjoyed usual health. A respectable physician, from Sheffield, gave an account of a very distressing fever, which had prevailed there since 1795. That a mill-dam was erected in 1787, to which it was by many ascribed; yet he declared, that from 1787 to 1795, great health prevailed, though the dam, during that period, was as high as it had been since. He also said, that during the spring of 1799, the dam was lowered, and that the disease, the summer following, was much more mild.

“It was admitted, that the exposing of vegetables or animals, or other substances, capable of being reduced to sudden putrefaction, to the sun, by drawing off water, draining ponds, or clearing up low grounds, tended to produce disease; but certainty, or even con-

nection, as to particular instances, in which this consequence had followed, seemed scarcely attainable."

"It was obvious to all the hearers of this trial, that the more proof, the more doubt, and that the question grew more perplexed, by investigation. And so fully were the court and jury impressed with this idea, that they decided in favor of the owner of the dam, and gave damages accordingly; saying, that they could not find it proved a nuisance."

In the note communicated by Dr. Ives to Dr. Hubbard, and contained in the letter already mentioned, is this assertion respecting New Milford. "In this town intermittents have prevailed more than in other localities along the river, often in that town, running into malignant remittents. For thirty years, intermittents had disappeared in that locality; but within three or four years last past, they have again appeared in that locality, but not extensively, or in a severe form."

Bethlehem. The Rev. Mr. Backus has given an account, in the Medical Repository,¹ of an endemic fever in that place, in the year 1750, which followed the draining of a pond. The physicians of that time, according to the accounts of old people, called it the plague. The Rev. Dr. Bellamy says, in his records, it was a nervous, long, but very malignant fever. In 1760, the ground was again overflowed, and after letting out the water, another wasting sickness, called a *malignant pleurisy*, carried off about forty inhabitants. The town was at the same time afflicted with influenza. But it is mentioned that the influenza pre-

¹ Hex. 1st, Vol. I.

ailed very generally in the years 1789 and 1791, although the pond had been drained, and reduced to a meadow for twenty years.

Dr. Boott, who cites this statement, considers it highly probable that the cases of malignant pleurisy in 1760 were due, as the fever of 1750 evidently was, to the Malaria from the drained pond, since such cases have been frequently found in winter, in places obnoxious to similar exhalations.¹

In all this there is no mention of any thing like intermittent fever. But as a remarkable instance of the effects of Malaria, in a region where this cause often does produce intermittents, I have thought it worthy of mention.

Litchfield, Conn. See page 49.

Sheffield, Mass. The first series of statements respecting the origin of intermittent fever in this place, which I shall bring forward, is obtained from two papers published many years since, by Dr. W. Buel, whose letter has already been adduced in the preceding pages.

One of these papers is "an account of the febrile disorders which prevailed in Sheffield, in the years 1793, 1794, and 1795,—and may be found in Webster's Collection of Papers, on the subject of bilious fevers;²—the other is an account of the bilious fever and dysentery, which prevailed in the same place, in the year 1796, and is contained in the New York Medical Repository.³ I shall digest and connect

¹ Life of Armstrong, &c., London, 1833, Vol. I, p. 570.

² New York, 1796, p. 53.

³ Hexade 1st, Vol. I, p. 453.

together the information derived from these two documents, respecting the localities in question.

“The river Housatonic runs in a serpentine course through this town, from north to south, with a very gentle current. Its banks, at low water, are from eight to fifteen feet high ; the bottom, for the most part, is soft and muddy ; and its depth such as to make it fordable in but a few places. On each side of this river is an extent of *intervale* or meadow land, averaging on both sides at about a mile in breadth. The greatest part of this *intervale* is overflowed at the time of the thawing away of the snow in the spring ; and sometimes by large and sudden freshets, at other seasons. The nature of the soil, in general, is such, that, very soon after the water is off, the land is dry and fit for tillage. It is, however, much interspersed with coves and marshes ; in the former of which, the water remains stagnant a considerable part of the summer, and in many of them perpetually ; of the latter there are not many upon this river, but there are some which remain such through the season.”—“The water contained in these coves, which are in fact great reservoirs of animal and vegetable filth, is, in the course of the summer, evaporated from some to dryness, from others nearly so, and from all in a degree proportioned to the dryness of the summer.”

“Beside the Housatonic, there are two other considerable streams, running through part of this town, which unite and empty into that river. Upon each of these streams, are large tracts of low, marshy lands ; great part of which is overflowed by freshets, and is never perfectly dry. There is (which is very material) a mill-dam on each of these streams ; and the two

dams occasion the overflowing of several hundred acres of the low lands. As summer advances, and the ponds fall, considerable parts of these lands are left uncovered by the water; more or less, according to the drought of the summer. In both of these ponds are large quantities of timber and other vegetable matters, which, in hot weather, are always in a state of putrefaction. This state exists in an increased degree as the substances become more exposed to the action of the sun. The fetor occasioned by this putrefying mass is such, in hot weather, and when the water is low, as to be extremely offensive to the smell, at the distance of many rods.

“The inhabitants of this town, who live in the vicinity of these marshy and drowned lands, have, as would naturally be expected, been always subject to remittent and intermittent fevers, from its first settlement. It is however, generally remarked by the old people, that these disorders have of late years, until the three last,¹ decreased; owing, probably, to the clearing, or partial draining of the lands.”——

“In the year 1793, during the season in which such complaints usually appear, intermittents and their concomitants were more frequent than they had been for many years before. In September and October, there were a few scattering instances of bilious fevers.

“Early in the spring of 1794, inflammatory complaints, chiefly of the pneumonic kind, were unusually prevalent. They were soon succeeded by intermittents, which were more frequent than they had been

¹ This paper is dated 1795.

the year before. Nothing peculiar attended them, and they continued to occur pretty often through the summer.

“Towards the last of July, the bilious, or, as it is here called, the pond-fever, began to make its appearance.”—“The disorder was chiefly confined to the vicinity of the south pond.¹ The influence of this pond appeared to extend about one mile and a half from its borders. Within this place there are about one hundred and fifty inhabitants; and about eighty of this number were affected with the fever; part of them inhabitants of Sheffield, and a part of Canaan. Among those who were sick, there were five or six instances of mortality. There were not more than ten or twelve persons who had the disorder in other parts of this town.”—

“The disorder was, probably, in all respects, what is termed a bilious remittent fever. It began with an ague fit; intense pains in the back, head and limbs, soon succeeded by thirst, dryness of the skin, &c. continuing without much variation eighteen or twenty hours; a slight moisture then broke out upon the skin, seldom a profuse sweat; a degree of remission of the fever, and abatement of the pains, then ensued, and continued till about the time of day of the first attack, when another exacerbation of fever commenced, with symptoms similar to the first. If the disease was left to itself, the remissions would sometimes become shorter and more imperfect, as the paroxysms

¹ “There were some scattered instances about the north pond, and others at a distance from both, probably due to stagnant waters about the Housatonic.”

were repeated, until it grew to be nearly or quite a continued fever."——

"It is unnecessary for me to say any thing more, in this place, than that assiduous purging, in the beginning, and a plentiful use of the bark, after the remissions had become such as to make it admissible, were the essential parts of the management of this disorder.

"So strong was the tendency to disorders of this kind, that people continued in some instances to be affected with intermittents, or fever and ague, through the winter. These were very frequent in the spring, but with no peculiarities, and yielding to the common remedy with the usual facility."——

Dr. Buel states, that during the month of August, 1795, he was absent in the western part of New York, where a disorder similar to that which he found on his return at Sheffield, was prevalent in similar situations.

"On my return to Sheffield, which was the 5th of September, I found a number of the inhabitants, about the north pond, afflicted with a fever, which began to appear about three weeks before. The people first attacked were those who lived nearest to the pond; whole families of whom were down at once. Numbers continued to be taken, daily, chiefly within the vicinity of this pond, or within three quarters of a mile of its borders, till about the middle of October; after which time there were few instances of new attacks. In this time, i. e. from the 10th of August to the 20th of October, of about 200 (which is not far from the number of persons living within three quarters of a mile of some of the borders of this pond), not less

than one hundred and fifty were affected with more or less of this disease ; out of which number, but one person died, and that an aged man, previously debilitated and disordered. The number affected with this fever, in all other parts of this town, did not, I believe, exceed thirty. Of these, three died ; one, an aged woman, the other two pregnant women ; of whom, one died in the fever, the other suffered an abortion, and died some months after, dropsical.

“The disease, this year, put on a different form from what it did the last. It might with more propriety be called an intermittent than a remittent fever ; though it was very different from a common fever and ague. It began, like other fevers, with an ague fit, attended with pains in the head, back, and limbs. The duration of this part of the paroxysm was uncertain. It was succeeded by a hot fit, whose duration was, in different persons, from six to forty-eight hours. A remission, and, sometimes, nearly or quite, a perfect intermission, then came on ; but whose duration was as irregular and uncertain as was that of the paroxysm before. After the first, the paroxysms were not generally ushered in by a regular ague fit ; only some slight chills were felt ; and these were irregular, both in degree and continuance. The length of the next succeeding paroxysms and intervals could, by no means, be calculated for, from the preceding : so completely irregular was this disorder. The fever evidently tended to an intermittent form ; but it could neither be called quotidian, tertian, quartan, nor by any other name used by authors to distinguish the different species of intermitting fevers.

The pains in the head, limbs, and back, were very severe, particularly in the latter, which were so universally intense, that the symptom might almost be considered as characteristic of the disorder. In the *primæ viæ*, flatulency was nearly a constantly attendant, and very troublesome symptom. Evidences of an increased secretion and excretion of bile, were generally present through the disease, but were particularly observable in the convalescence. Some degree of yellowness of the skin, which was almost universal, indicated a reabsorption of this fluid and a deposition of it upon the skin. This yellowness was in two instances, which I saw, very intense. A slight degree of delirium was very common, during the height of the fever. The appearance of the tongue was much the same that it was last year."——

"Purging in the beginning, and afterwards a plentiful use of the bark, appeared to me to be the most successful way of managing it," (the fever).——

"The bark did not suspend the paroxysms in this disorder, in as short a time as it does in common intermittents; but if the patient was properly prepared, and the use of it was persevered in, never failed to have the effect."

Dr. Buel states, that those whose fever had been suspended by the bark were subject to frequent relapses, and to long and lingering convalescence, which drew an odium upon the medicine, but he was convinced that this happened equally when the bark had not been employed. "The old people," he says, "in this town, who recollect the times when disorders of this kind have prevailed here before, and when the

bark was not at all used, inform me that those who were afflicted with them, were a long time in recovering."

He continues—"That marsh effluvia, to whose action the inhabitants of some parts of this town are subject, is the exciting cause, and is necessary to the production of the disorders in question, is beyond any manner of doubt. This is evident from their existing only where this influence extends. But something more is wanting; otherwise we cannot account for their prevailing in some years, and not in others. Every circumstance relating to the ponds,¹ and marshes in this town has, apparently, been the same, for many years past; and yet, very little of this form of disease has appeared, for ten or twelve years back, until the two last. We must either suppose a peculiar constitution of the atmosphere, occasioning a predisposition to these disorders, and coinciding with the local cause, or marsh effluvia; or that the marsh effluvium itself is, by some peculiarity of the atmosphere, wrought up to a higher pitch of virulence, and thus produces a higher degree of disease. I am inclined to admit the latter supposition, as I am convinced that the fevers which have prevailed here for two or three years past, and the common intermittent fever, are the same, only differing in degree. I have seen all degrees, from the mildest form of intermittents, to the most extreme of bilious

¹ "I have endeavored to discover the cause, why the sickness in 1794, was confined almost entirely to the south, and in 1795 to the north pond; but I can find no local circumstances to have existed which should produce sickness about one, and not about the other, in either of these years."

remitting fever. It is impossible to say, where the line of division shall be drawn. The disease this year seems to have formed a connecting link, between intermittent and bilious fever; and, were I to name it, I would call it a bilious intermittent. Should the intermittent fever, in its usual form, prevail next year, the disorder may be said, in the three years, to have been in regular gradations run through."

I proceed to make some extracts from the second paper; that on the bilious fever and dysentery of 1796.

"The part of the town in which the sickness prevailed, is almost a perfect level."—"Beside the meadows adjoining the river Housatonic, there are several other streams which run through large tracts of flat and very marshy ground. On one of these streams towards the north part of the town, is the mill-pond which appeared to be the common centre of the sickness in 1796, and the preceding sickly years.¹ This pond overflows a large tract of land, which was formerly covered with a luxuriant growth of timber, and other vegetable productions, and which are all now dead and in a state of dissolution, in consequence of the action of the water upon them." (The following statement is here given in a note. "It is important that it should be remarked in this place, that this pond, although it was originally raised about the time of the first settlement of the town, which is between sixty and seventy years ago, was, when the dam was rebuilt, about twelve years since, raised about seven

¹ See page 63.

feet perpendicular, by which means the water spread over a much greater extent of surface, than it had done antecedently to that time. It is on the land last flowed, that the substances, to which I allude, exist.”)

“Whenever a dry season occurs, the water recedes from almost the whole of the land last flowed, and leaves the whole mass of dead animal and vegetable substances lying on its surface, exposed to the action of a scorching sun.”

He mentions that the fetor arising from the surface of this drowned land, can at times be perceived at the distance of half a mile.

The spring of the year 1796 was cold, rainy and backward; but from July, the weather was very hot and dry, and there was very little thunder and lightning. In September, there were thick fogs almost every night, and it was remarked, that the musquetoos were more numerous than usual. About the 20th of September, when the sickness was at its height, the weather became suddenly cold and severe, but without any favorable change in the disease.

The first case of dysentery Dr. Buel was called to witness, was on the 6th of July. Several other cases soon occurred, and on the 20th of July, the bilious fever appeared, after which time it frequently occurred, so that both diseases now existed together. In a circle with a radius of a mile and a half, having its centre at a point on the south-eastern side of the pond mentioned, and comprehending about one hundred families, at least half the inhabitants were affected with one of these diseases. Without this circle, scarcely ten families were affected; within it, not ten were exempt. Of the one hundred and fifty individuals who

lived nearest the pond, not ten escaped. Forty-four persons who inhabited the circle described, fell victims to one of the diseases mentioned.¹

I need not follow Dr. Buel in the description of all the phenomena of the bilious fever. It is enough to give certain points which place the disease in relation with intermittents.

Thus "the symptoms of attack in bilious fever, differ very little, except in degree, from those in common ague and fever." The cold fit is violent, and accompanied with rigors and shaking. Sometimes the hot fit runs on "without suffering any abatement that may be called an intermission, or even remission." At other times, after being followed by free perspiration "the febrile symptoms gradually abate, and subside either partially, so as to produce a remission, or, more perfectly, into an intermission."

"The interval, too, which succeeds is of uncertain duration. The great irregularity attending the duration of the different stages of the paroxysms, produces, in different cases, all the varieties of quotidian, tertian, double tertian, quartan, and even some which it was difficult to reduce to either of the usual distinctions of intermittent fever. A regular tertian appeared to be the most mild and eligible form of the disorder, and that in which it was the most easily manageable.

¹ Dr. Buel mentions, in a note, that two or three of the deaths were of children, belonging near a mill-pond at the south part of the town, who died of dysentery; a fact which he has ascertained since writing this account. The prevalence of the fever in 1794, near the *south pond*, is ascribed to the waters having been drawn off in that year for the sake of repairs; while in common years its basin is kept much more constantly and completely covered with water than that of the *north pond*.

When the fever was quotidian, or double tertian, the intermission or remission was too short to afford an opportunity of doing much with the most important curative remedies. A regular quartan form was not frequent, although there were some instances of it."

Dr. Buel remarks, that the convalescence was remarkably protracted. It has been attempted, he says, to explain "the frequency of relapse in this fever, and in common intermittent fever," by the action of incidental causes.

"But it is evident," he continues, "that there is something in the nature of the contagion producing these fevers, which disposes it to operate on the human body, if I may so express myself, with an intermittent action. By intermittent, I do not mean to allude to the type of the fever, but the periods of the different attacks. The system, after it has become habituated to the contagion, and ceases to be operated upon by it for a time, at the end of a certain period loses that habit, and is again excited into morbid actions."

The writer observes, that this tendency to relapse belongs to the nature of the disease, and is not produced by remedies, as is shown by great numbers of instances which he has seen, both in bilious fever and common intermittent. He remarks, too, that the returns always incline to be periodical, that they are in some way affected by the lunar influence, and that they have a tendency to septenary periods.

"I have myself," he says, "had four successive slight attacks of an intermittent, at almost exact intervals of three weeks. I have known persons to be attacked regularly in periods of two and three weeks,

with relapses of bilious fever, for three, six, and even twelve months, without interruption."

"The symptoms of attack in a relapse, are similar to those of the original disorder, but generally less violent; the intermissions are more perfect, and the disorder, in every respect, approaches nearer to the form and degree of common intermittent fever."

After these statements respecting the bilious fever, follows an account of the symptoms of dysentery. Each of these diseases sometimes supervened upon the other in the same individual, and they seemed to be complicated, or exist at the same time, rather than to alternate.

Dr. Buel adds some remarks on the cause of bilious fever and dysentery, and on the question, whether they were infectious.

"That the stagnant waters in Sheffield," he says, "and the sickness which prevailed there in 1796, and the other late sickly years, stand in the relation of cause and effect, is, I think, a position which no person capable of reasoning, can withhold his assent to, after admitting, and candidly considering the facts which I have stated. I am sensible that new facts were not wanting to confirm a belief among physicians and philosophers, that marsh exhalations are a poison which most infallibly produces what are called bilious fevers. But, however astonishing it may appear, it is a fact, that many of the people who dwell in the vicinity of the stagnant waters of this town, and even those who have been the greatest sufferers in the several sickly years, disbelieve the local origin of their misfortunes, and strongly oppose all attempts to remove or lessen the force of their cause."

The writer was convinced, that neither the bilious fever nor the dysentery, was propagated by contagion from one person to another, even within the sickly territory. He mentions an instance, however, in which the evidence is very strong, that dysentery was carried by an individual from Sheffield to Woodbury, in Connecticut, and there communicated to many individuals, of whom nine fell a sacrifice to the disease. He contents himself with stating these facts, and leaves them unaccounted for.

A few words may be added respecting the treatment. After certain preparatory measures,—bleeding in some cases, purging with calomel and jalap, according to the strength of the patient; the force of the disorder, and degree of deviation from an intermittent form, &c., the use of the specific remedy was commenced.

“When the intermission became distinct, and in urgent cases, where there was only a remission, I exhibited the bark, in all cases, unless opposed by the prejudices of my patients or their friends. I began the exhibition as soon as the sweating fit had subsided, and continued it till the accession of the next paroxysm. From two drachms to one and one half ounce, was taken in an intermission, according to circumstances. A suspension of the paroxysm never failed to be the consequence, the patient having no more than one, two, or at most three, afterwards.” But the liability to relapse, and the absence of connection between this circumstance and the use of the remedy, is again insisted upon.

In the following year, 1797, as we learn from a

postscript at the end of Dr. Buel's account, the bilious fever again appeared within the circle described, and at the usual season. Between twenty and thirty persons who had removed to situations near the pond, were attacked with this fever, without a single exception. Those who had escaped the preceding year, were also peculiarly liable to attack. No cases, however, had proved fatal; the symptoms were generally less violent than the preceding year, and the fever yielded more readily to the bark, than in former seasons. Dysentery had not appeared in any instance. Dr. Buel was informed of two persons, not belonging to Sheffield, who, after residing a few days in the sickly territory, were attacked with bilious fever, and died at their own residences, where that fever had never been known.

Such is the evidence which I have found in the very interesting and valuable communications of Dr. Buel, who deserves the gratitude of the profession for having given us one of the best local medical histories which we have hitherto obtained from a New England practitioner.

In consequence of having seen a communication in one of the Journals,¹ by Oliver Peck, M. D., of Sheffield, in which a patient was spoken of as affected with intermittent fever, I addressed a letter to this gentleman, requesting farther information. The substance of the answer received to this letter will now be laid before the committee, who will appreciate its minuteness and fidelity.

¹ American Journal of Medical Sciences, &c.

"Not having ready access to the publications of the day, I do not know what period is referred to in Dr. Buel's letter in 'Webster's Collections,' but suppose it to be in 1796, when a malignant intermittent prevailed here, combined with an inflammation of the mucous membrane of the large intestine (dysentery). It was confined to a circle whose radius may be a mile or a mile and a half from a pond, north and a little west of the meeting-house; destroying fifty or sixty of the inhabitants before it ceased. This disease equalled, probably, in violence, many of the intermittents of the south of Europe, described by Torti, Senac, Alibert, and others. Dr. Buel informed me that the patients were frequently comatose, and if the coma occurred in the cold stage of the fever, it was frequently fatal, but if in the hot stage, they frequently recovered.

"The cause of the extensive and violent prevalence of the disease in that year, was attributed to the raising of the dam at the outlet of the pond, which, by order of the courts of law, was lowered, and there has been no occurrence of an endemic fever affecting so many within the sphere of its influence since.

"This is the locality alluded to in my communication to the American Journal; my patient had resided near it for many years.

"These are the general facts in my possession in regard to the endemic of 1796; Dr. Buel's communication will furnish a more ample statement if required.

"My personal knowledge of the locality extends no farther back than 1820. I am informed, however, that the disease prevailed sporadically between the

periods of 1796 and 1820, and it is thought there was more of it between these periods than there has been since. In that manner it has existed since 1820; in some years many cases occur, in others, few or none. It is attributed to the influence of the same locality; at least I know of none others in the town at present, capable of exciting the disease, although it is possible there may be such places evolving enough of miasm to affect the health of the inhabitants, if not to excite intermittents.

“There are within the town several ponds (three or four) made by raising a dam at a convenient place; in the summer the water is drawn off, when the bottom of what is in the winter a pond, is dry. These places have been always the foci of Malaria; what effect is to be attributed to the considerable extent of marshy land in other parts of the town, as well as to the numerous ‘coves,’ or pools of water existing in places where was once the bed of the river, it is impossible to say; at any rate their character is not so notorious as that of the ponds, or rather places where there were ponds in the winter, as above explained; the locality alluded to by Dr. Buel being worse than any others within our limits. The disease within my experience has been easily arrested by bark and its preparations, but it is very liable to recur in about a fortnight from the time of its interruption. The people here generally have a prejudice against the arrest of the disorder, imagining that they must have about a certain number of paroxysms at any rate, and that they are left more healthy if the disease is permitted to go on undisturbed, though bark was formerly, and quinine is at present frequently used, and as I think,

with benefit. The disorder affects new comers more especially, and more certainly the nearer they reside to the locality.

“In some years we have a considerable number of cases, and in others few or none, as before remarked, nor do I know of any peculiarity of season that occasions the difference, unless it be the circumstances I am about to allude to as affecting in 1820 the locality to which my attention has been principally confined.

“In that year, when I was a student, intermittents prevailed within the circle described by Dr. Buel and myself, as above, to a more considerable extent than at any time since, or probably since 1796; but in order to render the presumed cause more clear, I must, with your permission, more particularly describe the place in question.

“The pond is about half a mile in length, and from twenty to forty rods in width, with banks from seven to ten feet high on each side, from the almost level bottom of the pond; forming a basin, as you perceive, in which the water is confined in the winter by a dam erected at a place where the banks approach each other. The quantity of water is regulated by the height of the dam; in the summer the law obliges the owners to draw off the water. The bottom of the basin is then mostly bare, consisting of soft mud, and through it the channel or brook meanders. Supposing now a considerable fall of rain should happen, the low ground for the whole extent of the basin will be covered with water, to a depth corresponding of course to the quantity of rain that had fallen; in the course of a few days the water will subside and be confined to the channel or brook

alluded to, exposing in the warm season the soft and oozy surface to a hot sun. At this time, a smell that is considerably unpleasant and *peculiar*, exists in the immediate neighborhood. These circumstances frequently occurred in 1820, and in that year there was a greater prevalence of intermittent fever.

“Whether the water ever remains long enough to permit a crop of water plants to be raised, or not, I do not know, nor what effect their death and putrefaction would produce. The surface, after the subsidence of the water, as far as I have remarked, is bare, soft, foul and slimy, emitting an effluvium affecting the organs in a peculiar manner, which of course cannot be described. Three or four years since, there was a more considerable number of cases of the fever, occasioned by the dam being higher than usual that summer, so as to occasion a more frequent overflow, and preventing the speedy draining off of the water. Twenty or thirty years since, in the south-west part of the town, there was prevalent an intermittent caused by the dam of a pond being kept up in summer, so as to cause its bottom to be frequently covered with water and exposed afterwards to a hot sun. The disease was not malignant, but prevailed to a considerable extent near the place. (This is called Bush’s pond).

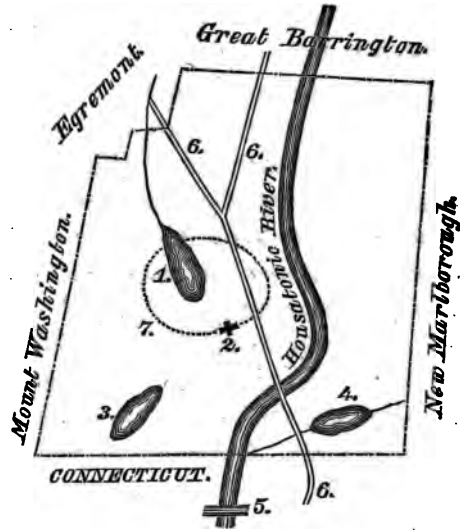
“In another place in the south part of the town (at Ashley’s), there was, thirty years since, a pond made in the same manner, and around it intermittents were very frequent until the dam was taken away. The bottom of the natural basin is now a meadow, and intermittents have ceased in that neighborhood since the change in these circumstances.

"Eight or ten years since a dam was thrown across the Housatonic in Connecticut, a mile or two below the line dividing the States; this raised the water in the river within this town, causing the low grounds and meadows to be more frequently overflowed. An intermittent of some extent followed, and prevailed for a year or two, until the dam was lowered by order of the courts of Connecticut; since then I have not known of a case near that locality, though there may have been now and then one.

"In Great Barrington, for some years past, they have had intermittent fevers, I cannot say to what extent. They prevail among those residing near the Housatonic, and are attributed by the inhabitants to the foul water of the 'coves.' (This fever has been more common in Great Barrington of late years than formerly).

"I am informed, but consider my information uncertain, that they have fever and ague in Stockbridge. In the year 1820, when we had an extensive prevalence of intermittent fever, compared with any year since, I was attacked by the disease in July, and although it was frequently interrupted by bark in substance, it did not leave me till some time in the winter. In the spring I had two paroxysms; since that time I have not had it, excepting in 1831, when two paroxysms occurred. The disease subsided without the use of any remedy. I supposed at the time that it was caused by my more frequently passing the locality than usual, during the summer of that year. The locality alluded to by Dr. Buel and myself, as being now the only certainly known focus of

Malaria, is called Hubbard's pond. Shall I add a sort of map of the several places mentioned?"——



1. Hubbard's pond, the locality spoken of by Dr. Buel and myself as the principal source of Malaria.
2. Meeting-house.
3. Bush's pond.
4. Ashley's.
5. Dam across the river in Connecticut.
6. Roads.
7. Dotted line showing the space occupied by the endemic of 1796, and within which space it has occurred sporadically since.

"I have been thus minute in detailing the information in my possession regarding fevers of the intermittent kind, and more minute perhaps than you would wish, because several questions of great speculative and practical interest have been of late years started in regard to their causation. I am convinced that nothing can solve them but an accumulation of facts minutely and accurately observed. The leading fact observed here has been, as you see, the

alternate covering with water and exposure to the sun of considerable surfaces of ground. During the exposure, I think there is an effluvium given off that is *sui generis*; whether this arises from the death and putrefaction of any particular vegetable, or whether it is owing to something peculiar to the soil which gives out during the drying process a particular exhalation, I cannot say, but it differs entirely from the smell exhibited in cities from masses of decaying vegetable and animal matter. On the borders of the salt water, along the slimy banks of the creeks where there is a mixture of fresh and salt water, there is often an intense discharge of an effluvium, but as far as I have known, it does not occasion intermittent fever, and it is certainly different from the exhalation at Hubbard's pond, and in other places where the Malaria prevails. This different effect on the organ of smelling must of course be owing to a difference of material. Its effect on my organs of smell is as different from other sensations of that sort, as the smell of a rotten potato is different from that of decaying cabbage."

Such is the evidence respecting Sheffield; evidence which may well astonish those who are in the habit of speaking as if fever and ague were never the offspring of Malaria of New England manufacture.

Great Barrington. Vide p. 79.

Stockbridge. See Dr. Partridge's letter.

West Stockbridge. See Dr. Partridge's letter.

Pittsfield. It was stated in Dr. Buel's communication to the Committee of the Massachusetts Medical Society, that intermittents were formerly very common in this place, but that they had become nearly

extinct, in consequence of agricultural and other changes.

The following extract is from a letter with which I have been favored from a gentleman of that town, and is sufficiently conclusive with regard to the fact in question.

“Between forty and fifty years ago, a mill-dam was erected near this village, which caused the water to *set back* and cover over more than one hundred acres of land, then clothed with its native forest trees of soft maple, alders, red ash, and other timber and shrubs peculiar to low alluvial lands upon the streams. Soon afterwards all the timber perished, then commenced bilious fevers, and the fever and ague, as it was called. It was very sickly; many died, all were alarmed. The owner of the mill was prosecuted, and the dam destroyed. There have been no indigenous cases of intermittent within my residence here, say forty years. The sickness above-mentioned ceased soon after the destruction of the mill-dam. I cannot ascertain certainly whether there were any cases of intermittent fever before the erection of the mill, but am inclined to believe that there were. There are many small ponds, lakes, swamps, and alluvial lands in town; when the town was first settled, I am of opinion that there were occasional instances of the disease.”

Dr. Childs, of Pittsfield, spoke to me in similar terms of this endemic. He mentioned that the dam was on the west branch of the Housatonic, and that the fever and ague prevailed in Pittsfield and its vicinity very extensively, under the quotidian, tertian, and quartan forms.

He also added, that within two or three weeks, in consequence of the removal of a dam, there had been more cases of intermittent fever than for twenty years previous. Some of these cases degenerated into remittent fever, or took on the malignant character; four or five, perhaps, remained quotidian until cured. Dr. Childs mentioned that he had seen recently half a dozen cases of similar fever in the town of Adams, some of them intermittent to the close, and fatal. It may be added, that the rivers were generally very low at that time (Sept. 11th, 1837).

This closes the evidence which I have to bring forward respecting the Housatonic. It has been seen that some of the facts stated, had reference, not directly to this river, but to different collections of water in its vicinity. These facts it would have been difficult to separate from the others, and as the ponds in the neighborhood of a large stream are generally in some manner connected with it, and must derive the character of their banks from that of the valley through which the river passes, the statements given cannot be considered much out of place in this connection.

THE CONNECTICUT.

The locality nearest the mouth of this river of which I have any notice, is Middletown.

In Dr. Miner's Essay on Typhus Syncopalis,¹ is the following passage. "As far as respects endemic diseases, Middletown, from its first settlement, has

¹ Philad. Medical Recorder, Vol. XII, p. 208. (Dated July, 1825.)

been one of the most salubrious stations in the United States ; and of the epidemics which, within the last twenty years, have so extensively visited New England, it remained long exempt. Except on the Housatonic river, and perhaps a very few places on the seashore, Connecticut is exempt from intermittents."

In the "History of the Yellow Fever as it occurred at Knowle's landing,"¹ (on the east bank of the Connecticut, about six miles below Middletown) it is said that this village was and is "remarkable for its salubrity and exemption both from endemic and epidemic diseases." In the same paper Dr. Tully says, "I have always believed that the casual occurrence of yellow fever in the small and healthy ports of New England, particularly of those on the river Connecticut, affords the most incontrovertible evidence of its foreign origin that is perhaps to be found in the whole world. In these places intermittents and bilious remittents, the endemics of southern climates, are unknown, except in a few instances, in persons who have recently arrived from places where such diseases prevail."

As the two last citations contain general assertions respecting the river and the State of Connecticut, I may here introduce the often-repeated statement of the late Dr. Nathan Smith.

"It has been suggested that typhus occasionally arises from marsh miasmata (Good, Study of Medicine, Vol. II, p. 188), the same which, under certain cir-

¹ In a letter to David Hosack, M. D., by William Tully, M. D., of Middletown, Conn. (N. Y. Med. & Phys. Journal, Vol. I, p. 153). See also Miner & Tully's Essays (Middletown, 1823), pp. 291, 359.

cumstances, produce intermitting and remitting fevers. A fact, which I shall here adduce, is strongly opposed to this hypothesis. On the Connecticut river, from Northampton in Massachusetts, to its source, a distance of more than two hundred miles from north to south, and on all its tributary streams, on both sides, for an hundred miles in width, there has been no instance of any person's having contracted the intermitting fever, from the first settlement of the country to the present time; and yet the typhus fever has prevailed more or less in every township within that tract of country."¹

Dr. Boott adopts and attempts to explain this supposed fact.²

Dr. Miner has written an essay of a few pages on the diseases of the river Connecticut,³ which, however, is deficient in the details we might have expected, and contains nothing satisfactory with regard to the present question.

I return to the different towns in their order.

Hartford. Dr. Webster has endeavored to show, from bills of mortality, &c. that this place is more healthy than Salem. There is no notice in his papers of intermittents.⁴ In Dr. Cogswell's sketch of the weather and diseases of this place during the winter and spring of 1798,⁵ it is said that there were early and immense freshets during the season; but notwithstanding this, the characteristic feature of all the

¹ Med. and Surg. Memoirs, p. 50.

² Memoir of Armstrong, &c. Vol. I, p. 473-4.

³ Essays on Fevers, &c. p. 84.

⁴ Hist. Coll. 1st series, III, 4.

⁵ Med. Repos. Hex. 1st, Vol. II, p. 301.

complaints was inflammatory, and it is added, which is deserving of notice, "the *bark* has almost lost its character with us."

Windsor is noticed in the Historical Collections, but nothing is said of its diseases.

Northampton. In consequence of having heard mention made of a trial arising in former times out of the consequences of the construction of a dam, I addressed a letter to Charles Seeger, M. D., of that place, who was kind enough to afford me the following information.

"It happened, in 1792, when a company, the most of them Hollanders, built the South Hadley Canal, between eight and ten miles below the centre of Northampton, to convey boats and rafts round the falls in Connecticut river, a dam was made at the head of the falls, eleven feet high, across the river, which raised the water for ten miles above about four feet higher than its common level. In consequence of this, the spring freshets flowed back much farther than before, and left large quantities of stagnant water when they withdrew. A great many of the inhabitants of this town, living and working near and amidst these low, marshy places, were for several years afterwards afflicted with the fever and ague, a disease which was unknown in this town for more than sixty years. Several of the inhabitants instituted suits against the proprietors of these works, under the nuisance law, which compelled the latter, some years after, to remove the dam and deepen the canal sufficiently to fill it without the aid of the dam. After removing this cause, its effect of course gradually ceased, and the town recovered its character of a

healthy place. The facts as to the origin of the disease, and its continuation from 1799 to 1803 were proved during the repeated trials, and many cases coming under my observation, I was called upon to inform the court and jury of what I knew of the causes and treatment of this disease.

"The type of this intermittent was generally tertian, but in broken constitutions and elderly people, it frequently changed into remittent fevers, and in some into various chronic diseases."

Mr. M., a gentleman of my acquaintance, residing in this city, has favored me with the history of a disease under which he suffered, while a member of the Round Hill School, in Northampton, and which appears beyond question to have been an instance of genuine quotidian fever and ague, originating in New England, and doubtless in Northampton.

He first joined the Round Hill School, he tells me, at the age of thirteen, in the year 1824. He had never been out of New England, had occasionally visited Portland, in Maine, and its environs, and had scarcely known sickness. It was mid-winter when he went to Northampton, and the disease made its appearance soon after the April vacation. The symptoms were, violent chills, with chattering of the teeth (so that he required to be covered with blankets), followed by fever, headache, and pain in the limbs. These symptoms appeared every forenoon, day after day, for as many as ten or twelve days, and in the afternoons, as he distinctly remembers, he felt *perfectly well*.

His physician (he believes Dr. Barrett) called his disease *fever and ague*. He took the bark in sub-

stance, and as he thinks, in wine, for a month or more. Although the paroxysms were suspended, he continued ailing all summer; and ever since this disease, he has been subject, whenever he takes cold, to severe chills, with chattering of the teeth, with pains in the limbs, and followed by fever. In 1833, while at Havana, he had several febrile paroxysms, he thinks three days in succession.

He does not recollect whether others had the same disease at the time when he was affected. He is not aware that there was any extraordinary local cause acting at Northampton to produce disease, but he remarks that the meadows are usually overflowed in spring.

Hatfield. See Dr. Partridge's letter. For Amherst and Sunderland, see Dr. Dorrance's letter. For Montague, see Dr. Lyons's letter.

Deerfield. Dr. Stephen W. Williams, of this town, has published two papers upon its medical and physical history, from which I shall make some extracts. The first is entitled "Observations on the Climate and Diseases of the town of Deerfield," &c. and was printed in the Transactions of the Medico-Physical Society of New York.¹ The second, which contains many of the statements of the preceding, may be found in the Boston Medical and Surgical Journal for November 2, 1836. From these papers I derive the information which follows.

"The climate of this town is not essentially different from that of the neighboring towns in the interior

¹ Vol. I, p. 62, (1817).

of the Commonwealth, from twenty-five miles from the seacoast to the banks of the Hudson river, in the State of New York. It is extremely variable, and has by no means the uniformity of climate of some of the southern and western States.

"This town may be considered as healthy as any upon the Connecticut river, and perhaps as any in the United States. The diseases are nearly similar to those of the neighboring towns in the vicinity of this place. There are now no local causes which peculiarly predispose to disease. Formerly the inhabitants were peculiarly subject to intermittent and bilious fevers, owing to stagnant waters and vegetable miasmata, which were occasioned by the subsiding of the waters from the level of our meadows."—"This fact is contradicted by the late Dr. Nathan Smith, in his work on typhus fever, who asserts that intermittent fevers never originate in the valley of the Connecticut river, from its mouth to its source, for the breadth on each side of it of forty or fifty miles. We have, however, abundant facts to show that fever and ague was formerly very prevalent in this town." (Added in the paper last published.)—"The site of the town, street, and extensive meadows, was the bed of a lake, a passage for which has burst through the mountain about half a mile from the place where it now empties into Connecticut river. Upon the subsidence of the waters of the lake, the meadows were left an extensive marsh, interspersed with numerous ponds of stagnant waters; the fogs arising from which, and putrid vegetable miasma, annually occasioned numerous cases of fever and ague. This is always the case in new level countries in the neigh-

borhood of lakes and ponds of stagnant water. Within sixty years,¹ however, there have been few cases of this disease, and at present it does not prevail here. The marshes have dried up. Deerfield river runs rapidly through the meadows, and there are no ponds of stagnant water."——

"Generally speaking, there are no prevailing disorders."——

"Once, within twenty years, we have seen the terrible ravages of epidemic dysentery ; and twice within that time, of a peculiar malignant epidemic fever. The origin of the spotted fever which prevailed here in the autumn of 1806, was traced to animal and vegetable putrefaction, and a want of proper attention to cleanliness in the families in which it prevailed. Its destructive ravages were confined principally to these families. A few sporadic cases have appeared since that time. The pneumonia typhodes we have had in common with our neighbors, in the different towns in the vicinity of this place. The other fevers which have recently prevailed here, have been either typhus or the autumnal bilious remitting fevers. These have been principally confined to that section of the town which lies immediately upon the banks of Connecticut river, which has hitherto been considered the most healthy part of the town. This tract of land is separated from the main street by a high ridge of hills running through the town from north to south, and terminating south at the Sugar Loaves."——

¹ " Fifty years." (First paper).

"It has been stated, and I believe the observation is correct, that diseases are generally less inflammatory in this place than in the neighboring elevated towns.

"Of late, our winters are more unhealthy than our summers. Fevers prevail more at this season than at any other, contrary to the usual laws of these complaints."——

"The Parish Register states the number of deaths in this town for twenty-nine years past, to have been 510. Upon an average, this is a fraction over seventeen a year. According to the same register, it appears that fifty-nine of these have died of consumption, sixty-six of dysentery, and forty-eight of fever. The accounts of the diseases are, however, taken merely from the recollection of the clergymen. Probably the deaths from fevers have been more than is registered. Many of the inhabitants of this town have lived to an extreme old age, and their deaths have been occasioned merely by debility from that cause."

The following letter, which I have received from Dr. Williams, I lay before the Committee with peculiar pleasure. From the researches of a few local historians like Dr. Williams, and the recollections of a few patriarchs like his correspondent, how much might be done to retrace the doubtful outlines of the past!

The letter addressed to myself, and the other letter which it contains, will be given without any alteration except the omission of a few sentences in the first.

"Deerfield, Mass., Feb. 3d, 1837.

"My Dear Sir,

"Yours of the 3d ultimo, requesting information concerning the fever and ague, which formerly pre-

vailed, in this town, in the valley of the Connecticut, and even in New England, is before me, and I should have replied to it during the month of January, could I have obtained the facts which I thought would be most interesting to you, during that period. At the time I received your letter, I was busily engaged in endeavoring to establish an antiquarian and historical society in the old town of Deerfield, in which, with the assistance of my friends, I have happily succeeded. I was satisfied, that in looking over the old letters and papers of some of our most distinguished ancient citizens, I should be able to find some written documents to substantiate the truth of my position that fever and ague formerly prevailed here to a considerable extent. I have found some written account of it, but get more from tradition upon which I can depend. In justice to Dr. Smith, I will mention that I was in an error in stating, that he asserted, that no case of intermittent fever had ever occurred or originated on the banks of the Connecticut, from its mouth to its source, since the first settlement of the country to the present time. It should have been from Northampton, north, for the distance of two hundred miles, or on any of its tributary streams. This assertion is equally erroneous, as I shall now endeavor to show.

“My grandfather, Dr. Thomas Williams, who was settled here as a physician from the year 1739, or 1740, to 1775, when he died, (see my memoir of him in the 4th Vol. of the Trans. of the Mass. Med. Soc.) writes to his wife from Albany, in June, 1756, when going to take the command of a regiment in the campaign against Crown Point that year, that he had just

had four fits of the *fever and ague*, between Westfield and Albany. He left Deerfield a day or two before, where he resided during the winter and spring ; consequently he took the complaint at Deerfield. The same year, Major Elijah Williams, of this town, wrote to my grandfather in the army at Fort Edward, that he was grievously afflicted with fever and ague. He always resided in this town. I have been engaged in the practice of physic in this town, about twenty-four years, and am nearly forty-seven years of age. No case of fever and ague has originated in this town since my own remembrance, although I have seen cases which have been brought here from the north and west. I have inquired of several of our aged citizens concerning it, and they have communicated to me the following facts:—My father, who was a physician in this town, has often told me, that his father, mentioned above, who also practised here more than thirty-five years, had often informed him, that fever and ague was formerly very prevalent here. My father also told me, if I am not greatly mistaken, that at the time of the erection of the dam across Deerfield river, at Stebbins's meadow, about the year 1793 or 94, fever and ague prevailed in several families living on the banks of the river at that place. This fact is also corroborated by Col. Asa Stebbins, of this town, now over seventy years of age, who was one of the proprietors of the dam. Col. Stebbins also informed me, that his mother, who was a native of this town, had the fever and ague, which she took here, two years in succession. Mrs. Bradley, a very intelligent old lady, now in the eighty-ninth year of her age, informs me, that when she was ten or eleven years of age, she

had a very severe attack of fever and ague, which lasted her all summer, and that an older sister was similarly affected at the same time. They lived near the banks of a marsh, or flag-pond, which was always stagnant and turbid, and often called toad, or toadle pond. She also mentions that fever and ague was very prevalent here, when she was a girl. Mr. Hawks, aged seventy-six, tells me that his father and mother both had fever and ague when they were young. They were both born and brought up in this town. He mentions that his father stated to him, that until the meadows were drained, a considerable portion of which were covered with flag-ponds, fever and ague, and other fevers prevailed here to a great extent. Very many of our aged people testify to the same fact, so that testimony can be found in abundance, to disprove the assertion of Dr. Smith. Mr. Robert Bardwell, of this town, took the fever and ague while boating on the river, between this town and Hartford, between the years 1795 and 1798. My friend Dr. Stone, of Greenfield, three miles north of this, will inform you of two cases of fever and ague which occurred in his practice in that town about thirty years ago."——

"I shall shortly present you facts to show that fever and ague formerly prevailed at Hatfield, five miles north of Northampton. As you ask for facts in relation to this complaint in any part of New England, I present the following from Stockbridge, Mass., about forty miles W. S. W. of Northampton. It is stated in the Historical Memoirs of the Hoosatunnuk Indians, published in 1753, that on the 13th of August, 1753, 'Mr. Sergeant, of Stockbridge, re-

ceived a letter from the Hon. Adam Winthrop, Esq., informing him that at a meeting of the Commissioners it was proposed and agreed to that he should be ordained at Deerfield, when his Excellency, Governor Belcher, should come thither upon a treaty, with the Indians of several tribes, who were to meet him at that place. The Indians of Hoosatunnuk being sent for by the Governor, set out for Deerfield about the 18th of August, but Mr. Sergeant was then in doubt whether he should be able to go, as he was taken a few days before with the *intermitting fever, the common distemper of all new comers* to Hoosatunnuk.' He, however, recovered, set out for Deerfield, and was ordained there on the Sabbath following. In an excursion to Sheffield, in Berkshire county, on the banks of the Housatonic, with my friend and colleague in the Berkshire Medical Institution, Dr. Delamater, in the year 1826 or 1827, he informed me that fever and ague prevailed there to this day. The river is sluggish there, and the banks low, and I think it a favorable place for the origin of it.

"I have delayed writing to you, partly for the purpose of hearing from my venerable friend, Dr. Oliver Partridge, of Stockbridge, formerly an active member of the Massachusetts Medical Society, now nearly eighty-seven years old, but still vigorous. He was born and lived in Hatfield till he was twenty years old. I wrote to him, and received his answer last evening. It is so curious that you will pardon me for detailing a considerable part of it. It gives the history of two or three cases."

‘ Stockbridge, 28th January, 1837.

‘ Doctor Williams,

Dear Sir,

‘ In May, 1771, I came ’ (from Hatfield) ‘ to reside in Stockbridge. I well remember of seeing in Hatfield cases of ague and fever (but not frequent), and one very singular, say in 1761 or 1762. A man, Mr. Nash (who lived across the road from our house), stout, active, and whose business disposed him to be abroad early and late, was, in very warm weather, taken with ague and fever ; and unwilling to be sick, he wished to rid himself of ague, and was told that if he would stew black pepper in rum, till soft, mash it, and apply it to his wrists and feet, and drink a gill of the rum so saturated with pepper, just as he began to feel the symptoms of a fit, go to bed, and have water by (but not too cold, tho’ cool) to drink plentifully, he would have such a sweat as to put an end to, the ague. He would try it, although his parents and neighbors thought it would kill him, and were very anxious as to the event. I do not know how much of ague he had, but remember that the fever, to appearance, was excessively high ; he as red as a coxcomb, and raved like a madman, so that it was as much as four men could do to keep him in bed, and covered ; continually striving and crying out, I will go, I will go, for it is hot as hell. It was a very hot day, I remember, and the doors and windows were open, the neighbors and even boys were desirous to see the event. If I do not misremember, he was sweated, and had no more of the ague, and did not recover fast to his usual health.

‘ After I came to Stockbridge, in 1771, I found that some had the ague and fever, but mostly those

who came from the hills or old towns to reside here, but in Great Barrington and Sheffield, those who lived within the extent of morning and evening fogs on the river; there were more cases of the ague and fever there than in Stockbridge.

'Some years before the year 1791, Col. Elijah Williams built an ironwork on a small stream near the village in West Stockbridge, and the dam raised a large pond which covered land on which were many trees and brush-wood, and this land became and continued afterwards a noted place of ague and fever, rarely any one failing of having it, who went to reside there, and people settling on the hills around did not escape it altogether. The ague and fever continued to be frequent there some years. The first generation born and growing up there, in many instances had it; the second generation, now grown, have been so free, as scarce an instance has been known with them. The place has been subjected to autumnal fever since the ague ceased, and [what have] been called bilious remitting fevers, with daily paroxysms, generally coming to a crisis in fourteen days, if not arrested in the beginning, often deranged in the height of the paroxysms towards the close, and nearly one quarter of the cases proving mortal. In the year 1795 the like fever was epidemical in this and the neighboring towns, and very sickly at said village, where I was often. (Note. I saw not or heard of a case of ague and fever among them). One night it was necessary for me to lodge in the village a few rods from my very sick patient, and about ten, was shown into a chamber which had not been opened that day to be aired; it was disagreeably warm, as

the night was very damp, and I fatigued. I undressed and lay down in my shirt, without a covering. At three o'clock was waked perspiring freely; to go and see my distressed patient; the air abroad felt to me disagreeably damp (tho' fair), and somewhat chilly. Two days after, I came to Hatfield (hearing that my mother was sick); the day after my arrival was taken (Aug. 17th) with said fever, which, (under old Dr. Hunt's care), formed a crisis the fourteenth day; was deranged only the two last paroxysms. I was much reduced, and recruited but slowly. I was able to ride on horseback, anxious to gain strength and return. I practised riding directly after breakfast. One morning (say near last [of] September), meeting with parson L., he says, Doctor, you ride too early; you ought to wait till the sun shines; for people from the hill towns, if any way feeble, unless they avoid the morning and evening fogs on this river, they will get the fever and ague. His caution was too late, for within three days I was well stricken with it, and with so much difficulty relieved, as not to be able to return home till the last week in November.

'About forty years ago a Mr. Smith, from the hills east, in Connecticut, bought a farm, mostly tillage land, in the south-west part of Stockbridge, bounding east on the river, (west on a hill), here raised some ways above and below said farm; no stagnant water near, fogs rare (except in calm weather), and more rarely reaching his house, on rising ground, so as to hide the morning sun. His wife of a sedate disposition, quiet and slow of speech, not readily disturbed, and rarely from home. After about two years (1802, I think), the occurrence took place. I lodged in Ty-

ringham, say about ten miles south-east from home; in the morning rode a few miles north-east into Haycock hollow, to see a patient, and return home down said vale through South Lee; between nine and ten, A. M., I met said Mrs. Smith riding south-east up said vale, as I supposed out only on a visit, and passed with only a good morning. I here was nine miles from home, and she nearly thirteen. The same day, at four o'clock, P. M., four miles north-west from home, I meet Mrs. Smith again, three miles north from her home. I stopped and say, where have you been to-day? You must have been a round-a-bout way to be returning on this road. She says, I do not know where I have been. A few days ago the *fever ague* took me, and I was told that if I would rise early in the morning, eat some crusts of bread and drink water, and take an horse, and crusts in my pocket, and ride all day all the roads I could find, which I never see before, and eat only crust and drink water, I should lose the fever ague. Well. Is this your fit day? Yes. Have you felt any ague? No—a little before I see you in the morning, I might have a little chill, but I did not regard it, the sun was so warm and pleasant. Any fever? No—but may have drank more water than common with my crusts, and felt pretty well all day, but now am some tired. Where have you been? I do not know. After I saw you in the morning I rode on, and coming to the hills, turned and came back, took a road, went on north, till noon or after, and turned about to find the way home—going right, I suppose? Yes—Farewell. Desiring to know the issue of the strange impression on her mind, with the exercise and diet, I soon after went and in-

quired as to the event, and found that she lost the ague and fever that day, and had no more of it. You will excuse the circumstantial prolixity of my letter to refute the assertion you mention. The cases of Nash and mine must be charged to the valley of the Connecticut, as I had passed West Stockbridge fever. The cases of Mrs. Smith and others, will prove that the ague has originated on the Hoosatonnic. The singularity of the remedies used for Mr. Nash and Mrs. Smith, induced me to a particular relation of them. If I collect any thing more in answer to your queries, I will write again. If you can read this scroll, it will not only please but oblige your sincere friend,

‘OLIVER PARTRIDGE.’

“This letter,” Dr. Williams continues, “written by almost a Nonagenarian, I consider a great curiosity, and as such have been at the pains to transcribe it for you. It contains a great many valuable facts for you.

“You will find a particular account of an endemic fever which prevailed in Sunderland a few years ago, written by my friend Dr. Dorrance, in the 11th or 12th Vol. of the Boston Med. and Surg. Journal.”——
“No cases of fever and ague occurred at Sunderland, and that fever was accidental, as it has been sometimes here, and in other places.”

Amherst and Sunderland. For the following information, I am indebted to the kindness of Gardiner Dorrance, M. D., of the former of these towns.

“I have been in the practice of medicine for ten years in the valley of the Connecticut; eight years in Sunderland, immediately on the river; and the last two in this town (Amherst). During that time per-

haps eight cases of intermittent fever have come under my care; but none of them indigenous. All, I think, were of western origin."—"In the years 1831-2, typhus fever prevailed very extensively in Sunderland. About two hundred cases of it occurred. But although some attributed it to miasmatic origin, I thought I could trace it to specific contagion throughout. The doctrine, I know, is somewhat unfashionable; but that disease I do fully believe was propagated by contagion. It prevailed most in the dead of winter."—"In Amherst, a settlement in the vicinity of some mills, was formerly very unhealthy. A fever, called here, the Mill Hollow fever, used to prevail there, years since, and I should think from what I can learn of it, that it was bilious, and probably remitting in its character. In a village in Hadley, where an extensive mill-pond, the summer past, was dry, for the rebuilding of a dam, a similar fever has prevailed. A very offensive smell from the decaying vegetable matter in the pond, was very perceptible to the inhabitants."

Greenfield. Dr. Stone, of that town, has favored me with these facts concerning it.

"In 1805 I had two cases of tertian ague originating in Greenfield. They were caused by the drawing off of a saw-mill pond in spring, later than usual. The stench from the pond was insupportable. One of the patients was a State pauper, who had not been out of the county for a dozen years, and who lived near the pond; the other was a hatter, about thirty-five years of age, of a slender constitution, who had not been out of Greenfield for several years, or certainly not into any region where fever and ague pre-

ailed. There were other cases of fever in the neighborhood, but not of the intermittent type.

"The dysentery was very mortal in 1802. Of 1200 inhabitants, 250 were affected with the disease, and fifty-six died. In June of that year, there was a flood which covered all the low lands in the vicinity, and this was followed by warm weather and southerly winds. The low lands lay to the south of Greenfield. That year there was no dysentery at Deerfield, but there was much of this disease the following year. The workmen who took care of the meadows were attacked with vomiting and diarrhœa."——

"When I first went to Greenfield, I recollect to to have heard old people say, that intermittent fever formerly existed here."

Gill. Dr. Joel Lyons of this town had the kindness to send me the following letter.

"Gill, January 30th, 1837.

"Sir,

"My friend, Dr. Edward Jarvis, has requested me in your behalf to answer several questions relative to intermittents, &c. And in answer I must say, that I never have had a case of *indigenous intermittent fever* since I have been in practice in this town, which is over twenty-nine years. Many years since I made an inquiry of our oldest inhabitants with a view of ascertaining that fact, and also of the old physician resident in town when I came, and who had practised for twenty-four years previous, and could not ascertain from either that there had been a single case of *indigenous intermittent fever*, and from all the inquiry which I have made of physicians and the inhabitants of Northfield, which is situated north and east, and

Montague south of me, on the river, the result has been the same. As to 'febrile endemics,' we have had fevers of several kinds, denominated by physicians, typhus, lung, and congestive fever; of the latter I had but a few cases, which happened in 1832-3; of the former but few cases since 1815. Dysentery has never been a prevailing disease but once since I have been in town, and that in 1810. It commenced in June, and ended in August; it was not very mortal. There are sporadic cases every year, and it sometimes proves fatal, but no part of our town or vicinity is more exempt than another, nor are there any places suspected of being more unhealthy than others."

Northfield. I owe the letter from which I am about to give some extracts, to the politeness of Dr. Edward Jarvis, of Concord, Mass., who was formerly a practitioner in the place in question.

"I never have," he says, "known any indigenous cases of intermittent fever either in Concord or Northfield. Nor have I heard of any such from the oldest people in either place.

"I found two cases at Northfield—both originated in the western part of New York, and were convalescent and returned to Northfield. After being supposed well, these men were exposed to cold or other cause of disease, and this sickness took the form of tertians, which were readily relieved by quinine.

"I had one case in Concord. My brother had the fever and ague twelve years ago in St. Thomas, West Indies, and recovered. In August last, he was attacked with continued fever, which, in course of four or five days, took the form of tertian, and then was relieved, as the others, by quinine.

"I have no evidence to show that fever and ague ever did originate in any part of the Connecticut river valley. The tides do not reach, I believe, above Hartford, about seventy miles below Northfield. So far as I am acquainted with the banks between Greenfield and Brattleborough, they are alluvial, elevated from ten to thirty feet above the low water, and, except in high freshets, rarely overflowed. The rise of the river is rapid, and the fall equally so, so that there would be very little or no stagnant water, or wet meadows, or low swamps, from which miasmata would arise."

Walpole, N. H. In the notice of this town, in the Historical Collections, it is said that there never was any extraordinary epidemic to check the increase of the population; but this is all the testimony I can offer.

Charlestown, N. H. Two letters were sent to the Committee of the Mass. Med. Society, by Dr. Weber, of this place.

In the first he says, "I have seen but few cases of proper intermittent fever, during the nearly thirteen years that I have resided in this place, and all of them might be traced to a foreign origin in the western parts of New York. In many, however, of our vernal and autumnal fevers of a gastric or bilious character, I have observed after the first week, in favorable cases, a disposition to a morning remission so considerable, as almost to amount to a complete absence of fever, and in such instances convalescence was speedily ensured by a few small doses of quinine or bark given during these remissions. I have seen also two or three cases of rheumatism of an intermittent character, in which the same remedies were of striking

advantage in breaking the daily returns of pain ; and intermittent headaches during the last two or three years have been tolerably plentiful. Most of these cases occurred in low, moist situations, or in the vicinity of marshy spots of greater or less extent."

The second letter is to the same effect; and perhaps the writer had forgotten having sent the first.

In this he says, "In somewhat more than twelve years, I have seen but about six cases of regular, well-marked intermittent fever, all of the tertian type. Though the first paroxysms occurred here, these cases could not be considered indigenous, since all the subjects had, in the course of a few months previous, been in the western parts of New York and other adjacent regions subject to intermittents, where they were probably infected with the miasm."

Hanover. Dr. Lyman Spalding has given an account, in the *New York Medical Repository*,¹ of the bilious malignant fever which appeared in the country adjacent to Dartmouth College in the summer of 1798.

In this article it is said, "The town has been heretofore exceedingly healthy ; never was it visited with any contagious disease till the last summer with the dysentery."

There is nothing in this paper which might lead to suppose that intermittents originated in this place.

Bath. There is no mention of its diseases in the *Memoir in the Historical Collections*.

Lancaster. It is said, in the notice of this town, in the same *Collections*, that twenty-eight inhabitants

¹ *Hex. 1st, Vol. III, p. 5.*

died in the year 1813; most of them of a prevailing fever; but of what character does not appear.

I have no facts respecting any town nearer the source of the Connecticut.

THE THAMES.

Excepting with regard to New London, situated at its mouth, and Pomfret, upon one of its tributaries, I do not possess any facts which belong to the region watered by this river and its branches. Of New London I have already spoken.

In the letter of Professor Hubbard, before quoted, is the following passage.

"I will now state the result of my experience as it respects my former residence, Pomfret, in Windham county" (Connecticut). "I began practice in that place forty years ago, and constantly resided there till my removal to New Haven. I have never seen a case of intermittent that originated in that vicinity. I occasionally treated cases that originated in the south or west; but that intermittents formerly prevailed there, I have undoubted testimony. I was acquainted with a very sensible woman, who died in Pomfret about fifteen years since, aged eighty-five. Her father lived in Pomfret, on the banks of Quinabogue river (which divides Pomfret from Killingly). She informed me that when she was young, her father's family were all affected for one season (summer, I think), with Fever and Ague."

TAUNTON RIVER.

Raynham, Mass. "From a careful inspection of the bills of mortality, which in this place have been

kept for more than twenty years past, it appears that the air is by no means unfavorable to health and long life."¹

In the account of Middleborough,² it is said that the Indians are very subject to hectic complaints, but there is no particular account of diseases.

Halifax.³ The longevity of the inhabitants is mentioned, without other evidence as to the state of health.

THE RIVER CHARLES.

I have already spoken of some of the towns near its mouth, but there are several others of which notices may be found in the Historical Collections.

Brookline.⁴ There is nothing said of fever and ague among its fatal diseases.

Waltham.⁵ The soil is said to be dry or elevated, and the inhabitants to enjoy an uncommon share of health. "There are few, if any swamps, marshes, or stagnant pools in the place. These are causes for the salubrity of the air."

Newton.⁶ "From the early settlement of the place, it has been remarkably distinguished for the salubrity of its air, and the health and longevity of its inhabitants." Among the diseases which have visited the place, dysentery and sore throat are mentioned, but there is no notice of the disease of which we are in pursuit.

Needham. Nothing is said of its diseases.

¹ Rev. Peres Fobes, in Hist. Coll. 1st series, III, 169.

² Ibid. Vol. II.

³ Ibid. 2d series, Vol. IV.

⁴ Ibid. Vol. II.

⁵ Ibid. Vol. III.

⁶ Account by Rev. Jon. Homer, in Hist. Coll.

THE MERRIMACK.

The general testimony of a practitioner so long and well known as Matthias Spalding, M. D., of Amherst, N. H., needs no remarks of my own to render its importance evident. I therefore introduce the statements concerning the towns upon this river, with the following extracts from the very exact and obliging letter which he has sent me, taking the liberty to omit some points which, though very interesting, would lead me beyond my limits.

“ Amherst, N. H., Jan. 21, 1837.

“ Dear Sir,

“ Yours of the 6th instant was duly received, and it would give me much pleasure if I could communicate any thing to aid you in the least, in your very arduous enterprise. But I fear I cannot ; I will, however, furnish you with such facts and circumstances as have occurred to me in the course of my practice, and as may seem to have a bearing upon any of the facts you solicit. If any thing can be drawn from them to your purpose, I shall be gratified.

“ You have made four general *divisions*, with their sub-*divisions*. Under your FIRST division, you ask, First, ‘ Have any indigenous cases of intermittent fever occurred in your own town or practice ? ’ Answer. *None*—supposed to be indigenous or genuine. Second, ‘ Have you heard any such cases mentioned, either by old inhabitants, or others, as having existed at any previous time, in your town or its vicinity ? ’ Answer. *None*—unless of very ancient date, in the early settlement of the place, and doubtful. Third, ‘ Can you inform me whether intermittents are known

along the banks of the Merrimack in any part of its course, or around any of the ponds, or near any swamps, marshes, or peat-grounds, in your part of the country ?' Answer. None ; and here I wish to observe, that I have been in practice between thirty and forty years, and within a few miles of the Merrimack river. I first commenced practice in Chelmsford and Lowell, some in Haverhill and Newburyport, and have extended it up the river as far as Concord, N. H., or higher, a distance of nearly a hundred miles. I was and have been acquainted with most of the physicians, old and young, along its course, and with the elderly people, and have conversed with many of them on the subject of intermittent fever, also embracing the *ponds, swamps, marsh, or peat-grounds* ; and never could learn by those I conversed with, *one single* well-marked case of *indigenous intermittent fever*, unless there might have been some *heard-say case*, in the early settlement of the country.

"Your SECOND division, 'If any such cases have existed, I should be glad to know'—Here let me state or answer more fully your first question. I said *none* supposed to be *indigenous* or *genuine*. I have seen several cases of intermittent fever, and had the treatment of some of them in this place, Amherst, which were said to have been contracted in the north-westerly part of New York, or near the lakes, towards Canada."

I omit certain details respecting these cases, and the replies to some conditional questions, rendered unnecessary by the negative answers already given.

Dr. Spalding speaks of dysentery having sometimes

prevailed in Amherst, perhaps to be considered as endemic, and in one season (the latter part of August, September, and a part of October, 1826,) proving very fatal, from following certain epidemic diseases. The following circumstances are too interesting to be omitted.

“The situation here which has often been noticed as having given rise to dysentery, is on high ground, the side or declivity of a hill, springy or moist in the early part of the season; but dry in the latter, especially in those seasons when disease prevails. Another place I recollect, so reputed for the same disease, that so long ago as when I was a practitioner in Chelmsford, in that part now Lowell, it was often told to me. It was on the banks of the Merrimack, at the head of the great falls, called the Pawtucket. The banks were very high, the soil dry and rocky. It was noted for dysentery when I lived there, in 1803–4, and before that. How it may have been since, I am not able to say; and why the disease should have been so prevalent there, I am unable to determine. It might perhaps have been owing to fogs resting upon that part of the stream, in the season for dysentery, or from the great spray arising from the falls, and condensing in the evenings and nights of those seasons. It was not noted for other diseases, that I recollect.

“With regard to remittents or typhus. In the years 1802–3, a few years previous to my residing in this place, I have frequently been told by the inhabitants, that a very mortal autumnal or typhus fever, prevailed in a particular section of this town, affecting whole families, and continuing, or holding on in the

same individual for thirty, forty, or fifty days or more, with some of them. Why it should have been so, or what might have been the cause, I am not able to conjecture. There have no such fevers existed or prevailed in that section of the town since.

“Second. ‘Are any places in your vicinity reputed unhealthy ; if so, what places, and for what reasons?’ Answer. There are one or two, and as they are circumstanced somewhat alike, I shall trouble you only with one of them. This place comprises a pretty large tract of low, swampy, or marshy land, through which is a small brook struggling to wind its way ; but so feebly, on account of the flatness of the land, as to leave in its course considerable stagnant water, particularly spring and fall ; and it has long been noticed that those who live nearest to its margin, are afflicted with sore throats, tubercular swellings about the neck, vulgarly called king’s evil ; sometimes the lungs are known to suffer in this way. I know of no other reason for these complaints, or diseases, than its low, cold, damp situation, or perhaps some vegetable miasmata may exist. But I have not observed fevers to be more common here than in any other section of the town. I know of no other local disease in this place than those I have mentioned, and it may be considered upon the whole, as healthy as any one in New England.”

I proceed to mention some of the towns of which I have found notices, which are situated upon the Merrimack.

Haverhill, Mass. There is a long account of this place, containing many local and historical details, in

the Historical Collections,¹ but there is no allusion to the disease in question.

Lowell. This place has been already mentioned in Dr. Spalding's letter. My friend, Dr. Coffin, of Lowell, who practised some time in Nashua, before residing in the former place, was so obliging as to make some inquiries upon the subject of intermittents, in my behalf.

He writes to me, "During my residence in Nashua, I neither saw or heard of an intermittent, and I am pretty sure a case was never known in that vicinity." — "I have questioned some half dozen of the first men here. They have never seen an indigenous case in Lowell or vicinity. Dr. Dalton had one which was *born* in New York, Dr. Huntington another in a woman from the western part of Vermont, but she had been across lake Champlain into New York, where she contracted the disease."

Tyngsborough.² There were about sixty cases of spotted fever in this town in 1813, but no other distressing epidemic has visited it since its incorporation.

I here leave the main stream, to mention some facts respecting two towns upon one of its tributaries, the Nashua.

One of my correspondents has had the politeness to transmit to me a letter from Dr. Samuel Emerson, of Kennebunk, which was communicated to him in consequence of inquiries undertaken at my request. I derive from it the following interesting statement.

"Your request brings to my recollection an important historical fact, which ought not to be suffered to

¹ 2d series, IV, 121.

² Ibid. Vol. IV.

go down to the shades of oblivion. When I was a pupil of old Doctor Oliver Prescott, in Groton, in the county of Middlesex, when visiting a patient in Pepperell, the next town to Groton, and bordering on the State line, we passed a small river, called Nissitisset by the Indians, and which still keeps the name. This beautiful stream has its rise in a pond on the northern side of the above-mentioned State line, in the town of Brookline, in N. H., called Mosquatannipus, meanders through a very rich valley seven or eight miles, and pours its limpid waters into the Nashua. This short description of the geography of that little stream, though apparently irrelevant to the answer to your letter, yet will be explained in the sequel as necessary and important. In the course of our professional ride, the Doctor entertained me highly by the following account :—When I was a young man, and but just commenced practice, I visited an old and highly respectable physician, then living in Concord. Being a distant relative, by the name of Abel Prescott, he was kind enough to take a deep interest in my success in acquiring medical eminence and prosperity. He says to me, ‘Kinsman, a great proportion of my practice has been in intermittent fevers, for thirty or forty years ; one third part of your business, at least, is the same ; but the time is not far distant when this section of the country will be visited by a very fatal malignant fever, after which, the fever and ague will quit this part, and probably all New England, for ever.’ The event proved the prediction to be history. A man lived upon Nissitisset river, about the central point from its source in the pond, and its exit in the Nashua. He owned a rich

tract of intervale land covered with a poisonous shrub called dogwood, or white sumach (the Linnæan name I do not recollect). The proprietor being subject to eruptions, from working among ivy or dogwood, built a dam across the river, in order to flow this intervale, and clear it from the deleterious vegetable he so much feared. This flowing was continued long enough to effect his purpose, and then drawn off early in summer. The dead brush was cleared away, and the sun let in upon the rich soil. In a short time after this, the man, his wife, and several children were attacked by a disease which the Doctor called a *putrid malignant nervous fever*; the vulgar name was the Pepperell fever, from the place of its origin. This horrid distemper began its attack with a high degree of inflammation of the brain, and raving delirium, which made short work of every member of the above family, spread rapidly in the vicinity with equal fatality, and extended through a great part of Middlesex in Mass. and Hillsborough in N. H. The Doctor informed me that he lost every patient for some time, and nothing that he could oppose to the progress of the deadly ravager had any effect, till, being called to a girl about fourteen, he applied a large epispastic to the back of her neck; her tossing and struggling through the night, notwithstanding the best exertions of faithful watchers, kept a constant motion from evening, when it was applied, till near morning, when she lay still and fell asleep. Upon examining the blister in the morning, the Doctor found a complete vesication the whole length of the spine. This was the first patient that recovered. From this the Doctor shaped his course, and lost few

or none afterwards. My father's house is only three miles from the spot where this disease originated, and I have his testimony to the above facts, which took place in the year 1760, which was four years before my birth; and from that period the object of our inquiry has never appeared, and I can truly assert that I have never seen a case of pure intermittent except those which have been imported from a warmer climate.

"I urged Dr. Prescott to write and preserve a faithful detail of this interesting piece of medical history before his death; but he never did. After his death, I repeatedly requested his son, who was two years my senior, but never could induce him to undertake the work, though he felt the importance of it as much as I did. After the death of my very dear friend and fellow student, there remained no one but myself; and I am very glad that you, my dear sir, have put me up to my duty, and I wish it was done in a better style, though not much ought to be expected from an old man of seventy-two. There is nothing to recommend this honest relation of facts but truth, which I have as carefully adhered to as the strong impressions upon my memory would enable me to do."

The following fact is from the communication of Dr. James Jackson, of Boston, before referred to.

"The late Judge Samuel Dana, of Groton, stated to me about five years ago, that he had received from his father, or possibly his grandfather, who, as I understood, was formerly a clergyman in that place, the following information. He stated that when he first settled in Groton, intermittent fevers prevailed in a

certain part of the town, which was described as being at that time wet, but subsequently drained and cultivated. Judge Dana entered into some particulars on this point, and he evidently understood the subject of which he was speaking, so that it left no doubt in my mind as to the nature of the disease."

Concord, Mass. This town is situated on one of the tributaries of the Merrimack, and is therefore introduced in this connection. The North river, as it is called, also passes by or through the town. From a description of the place, by Mr. William Jones, student of Harvard College,¹ I make the extract which follows.

"Both rivers overflow their banks after any considerable rain; and in the spring of the year, when the snow melts, they cover the neighboring meadows, which are very extensive.

"The town, being surrounded with hills, appears as you approach it, to lie low, from which circumstance, together with the rivers and the vast tracts of meadow grounds which lie upon the rivers, persons unacquainted with the town might be led to imagine that it is an unhealthy place; but facts, however, prove the contrary. Those diseases which are peculiar to low marshy soils, are seldom or never known here."

Mr. John Farmer, of Concord, New Hampshire,² a gentleman well known to the public by his historical researches, addressed a letter to Thomas Chadbourne,

¹ Hist. Coll. 1st series, I, 237.

² The capital of the State, situated about fifty miles above Concord, Massachusetts.

M. D., of that place, whose reply he sent me in answer to a series of questions I had submitted to his attention.

A few of the most important of these questions, and of Dr. Chadbourne's answers, I here introduce.

Have any indigenous cases of fever and ague occurred in Concord or its vicinity?

"There have not. We have had many such cases to treat in this town, but they originated elsewhere."

Have you heard any such cases mentioned as having occurred previously in your neighborhood, by old inhabitants or others?

"None that could be relied on as distinctly marked cases of the disease."

Have you heard of such cases in New Hampshire, and particularly along the course of rivers, &c. &c.?

"None at all. The exhalations from our rivers and stagnant waters sometimes produce fevers, but of a different type from intermittent fever. They generally assume the character of the low typhoid, or continued fever of authors."

Are any particular spots in your vicinity, or in New Hampshire, noted for unhealthiness?

"There are none. There is probably no State in the Union that enjoys a greater exemption from disease than New Hampshire. The angina maligna, or throat distemper, visited some of the lower towns in the early settlement of the State, and more recently it has been visited, in common with most of New England, with the spotted fever; but no general epidemic has ever, to my knowledge, extensively prevailed in the State."

Boscawen. In the notice of this town, in the His-

torical Collections, it is said that pure air and uniform temperature generally prevail, and are conducive to the health of the inhabitants ; and that there are no morasses or stagnant waters.

The following extracts are from an elaborate communication which I owe to the politeness of Job Wilson, M. D., of Franklin, N. H.

"I have been informed by aged people, that about eighty years past, say in the year 1750, in marshy places near the sea, intermittent fever did actually exist in this State ; the circumstances of which were so particularly related that I have not the least doubt but that it was actually the case.

"I never saw nor heard of a case in this town, nor in this part of the State, nor on the Connecticut river in this parallel of latitude. This part of the country is mountainous and hilly, and in general somewhat dry ; it is not swampy nor marshy, though we have places which we call swamps, but they are usually of small extent, and are generally dry in summer ; the rivers are rapid, and their shores almost always dry in summer, and not at this day at all marshy. This situation (namely, Franklin and the adjoining towns) is relatively high and dry ; we have generally but little fog.

"In the neighborhood of this place lies Winnipiseogee lake and bays, which are considerable collections of water ; in some places the shores of these bays and the lake are flat, and at times covered with water ; but are usually dry in summer. About thirty years past the settlements about these waters were new, and the marshy parts less cleared than they now are ; it was sickly on the shores of these waters, and

diseases were said to have been more frequent and fatal than elsewhere ; it is likewise said to have been the case on Blackwater river in the town of Salisbury ; the shores of this river are somewhat marshy and swampy ; the diseases were the autumnal bilious fever and dysentery. Whether this was a modification of intermittent fever, with the autumnal bilious fever, I am not prepared to say ; I was then in the study of medicine, but saw none of the cases. Our autumnal bilious fever is a remittent, with two paroxysms in twenty-four hours ; the evening paroxysm is in general the most considerable ; the fever fit is in general preceded by more or less of a cold fit, but after two or three of the first fever fits, this is less observable, and is not often noticed.”——

“In respect to the flowing of our rivers, bays, lakes, &c., by mill-dams, there is no very decisive evidence as yet of their bad effects ; though some have been suspected of having a bad effect on the health of the inhabitants immediately in the vicinity, particularly in the town of Bristol, about fourteen miles above this ; but of this there has been but one instance of very distressing sickness, and that was about ten years past ; since that time, though the place, I should suppose, had been rather more sickly than common, yet nothing very remarkable has occurred. Immediately in this vicinity (in Franklin, N. H.) the river is raised by several mill-dams, but the water is so rapid, the shores are so bold and rocky, the dams almost always full or nearly so, that I do not see why it should at all affect the health of the inhabitants.

“ On the bays and lake Winnipiseogee, all or most

of which have been flowed by mill-dams, I have heard of no very serious injury ; though the shores in several places have been overflowed, but the water is always about or near the same height ; so that the flat ground is almost entirely covered with water, and not acted on by the heat of the sun. In long and very dry summers, should the water by any means be drawn off, so as to expose the flat grounds which have been lately overflowed to the action of the sun, there is reason to conclude that sickness would follow. In all the places above-mentioned, there are other causes which may have contributed to the production of disease, viz. great and long continued heat, as is the case in our longest and hottest summers, and is particularly the case on Blackwater river, both in Salisbury and Boscawen ; in these places the heat of the sun is very powerful at times, from the natural formation and southern declivity of the land on the shores of this river. See my dissertation on the autumnal bilious fever, published in the last numbers of the New York Medical Repository."

RIVERS OF THE STATE OF MAINE.

With regard to these rivers, as to this State generally, my information may be reduced into a very narrow compass. Those who know the difficulty of obtaining facts respecting long-settled and cultivated regions, may readily perceive the still greater obstacles to ascertaining those which relate to a vast territory, thinly inhabited, and that mostly by the pioneers of the forest.

I may with propriety introduce in this place the testimony of General Lincoln regarding the counties

of Cumberland and Lincoln, since they are bordered or traversed by several of the principal rivers.

"The counties of Cumberland and Lincoln, from their situation in the centre of the temperate zone; the purity of the air; the frequent gales; the nature of the soil; the height of the lands; the balsamic quality of many of the trees in the forest; the rapidity of its streams; their exemption from stagnant waters, poisonous animals, and noxious plants; the temperature of the weather in summer, and the regularity of the seasons in winter, are rendered equally, if not more healthy, than any part of the United States; and probably, in this respect, they are not exceeded by any climate whatever."—"An idea that the lands are generally covered with fogs, has probably deterred some from becoming adventurers in them. The fogs frequently extend over the islands, and a small border of the main next the seacoasts; so that if they were really prejudicial to the country, a small part of it only is affected by them. But it remains a doubt whether they are injurious or not; people on the islands are equally, if not more healthy, than are those inland, where they are not exposed to fogs; and the soil appears to be equally productive, at the least, on them as on the main, and the grain not more liable to blasts."¹

The letters from which I make the extracts which follow, I owe to the kindness of my friends, Professor Barnes, of Waterville College, and Robert Hallowell Gardiner, Jr. Esq., of Gardiner, Maine.

¹Observations on the Climate, Soil and Value of the Eastern Counties, in the District of Maine, &c., by the Hon. Gen. Lincoln, (Hist. Coll. 1st series, I, 142).

THE ANDROSCOGGIN.

Professor Barnes mentions that "A physician who resided some years on the Androscoggin, say twenty miles above tide-water, knew of no case in his practice or acquaintance. He had heard of one case resembling intermittent, of which he promises me a farther account, though he himself doubted its character."

A short time after this dissertation was sent in to the Committee, I received from Professor Barnes the following letter, addressed to him by a medical practitioner of highly respectable standing.

"Portland, March 30, 1837.

"Dear Sir,

"I received a letter a few days since, from Dr. Purinton, making inquiries respecting what I knew of intermittent fever occurring in Maine, and requesting me to write you upon the subject. I never knew of but one case that originated here; and the history of this case must be somewhat imperfect, as I made no notes at the time. The subject was an old gentleman, aged about sixty. His occupation was that of a farmer. Temperament bilious, habits regular. Had been subject to attacks of hepatic disease for several years before I knew him. He had taken mercurial medicine, and had been salivated repeatedly. He generally had one of these attacks in the course of the winter, for several winters. In the summer season he enjoyed very good health. As usual, in the early part of the winter of 1830, he was 'laid up' with his 'old complaint.' The usual remedies were used with their usual success for a while, but towards spring he got worse; mercurials failed to afford much relief. He became jaundiced; the whole surface of the body and limbs appeared as though he had been dipped

in a solution of gamboge. About this time commenced a new train of symptoms, viz. fever and ague. I have had the care of several cases that originated in the western country, and I have not been able to see any difference betwixt these and that of Mr. Stinchfield, of Poland. His fits came on exactly at one o'clock in the morning, went through the cold, hot and sweating stages as regularly and *beautifully* as any case I have ever seen. Dr. Delamater, then lecturing at Brunswick, saw him with me, and concurred with me in opinion that it was a genuine case of quotidian ague, *originating in the State of Maine.*

"Mr. Stinchfield's house stood very near an extensive fen, or bog, which always contained stagnant water. Dr. Delamater thought this might have contributed to make out the case. Perhaps so, but why did not others living equally near it take the same disease? Because they were not equally susceptible; in them the predisposition was not so strong. He died.

Yours respectfully,

J. W. MIGHELS."

THE KENNEBECK.

Mr. Gardiner writes to me thus: "Dr. Hubbard, of Hallowell, in answer to my questions, says that he has had many cases, but does not remember one where the disease had not been contracted out of the State; though he has had many cases allied to fever and ague, yet not entirely genuine." "Dr. Mirick" (of Gardiner), "agrees with Dr. Hubbard, that it is not indigenous. Dr. Nourse, of Hallowell, and Dr. Gage, of Bangor, maintain the same." A single case is mentioned in Mr. Gardiner's letter, supposed to have been an instance of indigenous fever and ague;

but as there was a difference of opinion among the physicians, I shall not use it as evidence.

In Dr. Hale's description of the spotted fever, as it prevailed at Gardiner, in 1814,¹ there is no allusion to intermittent fever, either under the head of the "Prevailing Diseases" of the place, or of the "Sketch of Diseases from 1813 to 1817," or of "Metereological and Pathological tables," or of "Other diseases" which existed at the same time with the epidemic.

Professor Barnes writes (from Waterville, on the Kennebeck), "I have traversed this State extensively, and have resided for considerable periods in several different parts, yet never heard a case mentioned. The principal physician here, who has had a large practice for twenty years, has never known an indigenous case. The oldest physician, who has lived here forty years, is equally ignorant of any but imported cases. One of our citizens has lived on the Kennebeck sixty-one years, in early life at Gardiner, twenty-six miles below this, latterly here. He was formerly a trader, and for a long period in the early times, when every thing except fever and ague was suffered by the settlers, he knew personally every man on the river, above and below. He told me at first that he knew a native case at Gardiner about 1775 or 76, but on farther reflection was inclined to doubt if it was not imported, and besides this remembered no other. These testimonies perhaps will do for the Kennebeck."

THE PENOBSCOT.

My last correspondent continues in a second letter, "I remarked before, that the most likely local-

¹ History, &c. Boston, 1818.

ity seemed to me to be the Penobscot, for some distance above Bangor, where the banks of the river are quite low ; so much so, that freshets are rarely disastrous on that river, the rise of the water being checked by the overflow of its banks. I wrote to a very intelligent young physician of my acquaintance at Old Town, from which place his practice and information extend over nearly the whole of the country I have described. I quote from his reply. 'I have never seen or known of any case indigenous in any part of this State. I have met with several cases of intermittent fever in this section of the country, but always found they had been contracted in a southern climate. While reading my profession with my brother in Bangor, I recollect a gentleman called for advice who was laboring under this disease. I inquired of my brother if that disease ever occurred indigenous in this climate. He said he had never known a case ; that this patient was from the south, and was attacked there. I have inquired of other physicians on the subject, and they say they have known no cases originating in this State.'——

"This writer is James C. Bradbury, M. D., of (Old Town) Orono. He adds some account of the prevalence of fever of typhoid character in those parts, in the first years of the settlement, which was ascribed to local causes, connected with the clearing of the soil, &c.

"I am sorry not to have received any intelligence from the Schoodic (St. Croix) waters. There are but very scanty settlements there, however, above the tide."

FACTS RELATING TO REGIONS BORDERING UPON LAKES AND PONDS.

I KNOW of but one part of the country properly falling under this division, which is obnoxious to the disease in question. This is the neighborhood of lake Champlain.

“Intermitting and remitting fevers are very common in the western part of this State, adjoining lake Champlain. They extend but a little way from the lake, unless in the vicinity of low and wet land. They are not so common as in former years, soon after the clearing of the land. They are not met with in the eastern section of the State, unless some solitary cases contracted by travelling in the neighborhood of the lake. It has frequently happened that people from the eastern part of the State, residing only a short time in the vicinity of the lake, in the summer season, would experience the disease; and after appearing to be perfectly free from it through the winter, perhaps for six months, and residing all the time in the eastern part, would have another attack in the spring. These second attacks were often very obstinate.”¹

Dr. Mann, in his Medical Sketches, says, “Intermittents showed themselves in a few instances during the months of February and March, 1814.”² This was at Burlington, but where the disease was contracted does not appear.

¹ Gallup's Sketches of Epidemical Disease in Vermont, p. 48.

² Med. Sketches, p. 128.

The same author speaks thus of that portion of Vermont included between the lake and the mountains. "From the bases of the mountains to the lake, which is the west boundary of Vermont, the distance is twenty miles. This tract is under good cultivation ; the soil rich and very productive in grass, every species of grain, and fruits. The whole of this district is in an improving state."¹

This corner of New England hardly seems, in fact, to belong to it, according to the natural divisions, but rather to form a part of the lake district of New York and Canada.

Two of the towns on lake Winnipiseogee are mentioned in the Historical Collections, but there is no allusion to their diseases.² I am obliged to close this division of my subject, for want of further materials, with the following extract from one of the letters of Professor Barnes.

"The only large lake about which there are settlements, is Sebago pond, in Cumberland county. From this pond the Cumberland and Oxford canal passes to Portland. The descent of the canal is very rapid ; I regret to have forgotten the number of locks, but it is very large for so short a distance. This, you know, is material. Haskell Pierce, Esq., of the Legislature, a very intelligent man, resides at the head of Long pond, a considerable lake above the Sebago. He knows the disease, having seen a good deal of it in the western country. He assures me that nothing of the kind is known in his vicinity, nor on the shores of the Sebago, nor on the line of the canal, with all of which section of country he is very familiar.

¹ Med. Sketches, p. 168.

² See also Dr. Wilson's letter, p. 118.

“Issachar Snell, M. D., of Augusta, of very high reputation throughout this State, has never known any indigenous case. He practised for many years at Winthrop, about ten miles west of the Kennebeck, a region abounding in small lakes ; latterly at Augusta. He has seen some chronic affections of the lungs, which simulated fever and ague, but his attention had been repeatedly drawn to the exemption from this disease in our State, and he was very clear in his opinion.”

FACTS NOT EMBRACED BY ANY OF THE FOREGOING DIVISIONS.

I HAVE proposed to assemble under the fourth and last head of my dissertation, such facts as are not embraced by any of the foregoing divisions.

On referring to the map which accompanies this paper, it will be seen that a number of towns not hitherto mentioned, are marked as having been the subject of notices or memoirs, and that one among them is considered as having given origin to intermittent fever. The greater number of the towns referred to, will be seen to be in Massachusetts. And I may here introduce the assertion of a writer, whose name is well known to the students of the medical history of New England ; an assertion which would have had more interest, if accompanied with a reference to the sources from which it was derived.

“With the same extensive improvement, their inhabitants will not be scourged by diseases more than those of the eastern States ; the first settlers of which (if historians and the most aged of the inhabitants are to be credited) were subjected to the same diseases, which are now endemic on the north-western territories. *Intermittent fevers*, one hundred years ago, were common in the lower towns of Massachusetts, where a single case has not been known to have originated within the last fifty years. The few autumnal fevers which are sporadic, generally may be traced to well-known exciting causes, independent of atmospheric influences arising from local positions.”¹

¹ Mann's Medical Sketches, Dedham, 1816, p. 110.

The single locality where intermittent fever is said to have originated, to which I have just before alluded, is Hopkinton. Dr. Stimson has given a *mémoire* of this town in the Historical Collections,¹ from which I derive the information which follows.

"This town is situated thirty-two miles westerly from Boston. There are two ponds in the town, from one of which issues one of the extreme branches of the Concord river, and from the other, one of the extreme branches of the Providence river. These ponds are both in the westerly part of the town. One of the extreme branches of the Charles rises in its southerly part.

"The town is hilly, interspersed with small valleys, and well watered. Some of the swamps, that have never been cleared and cultivated till within a few years, are found to be the most productive, and some of the best lands we have."——

"We have no instance of longevity exceeding an hundred years, though many have arrived to nearly that period. The town, however, is generally healthy.

"The weather is very changeable, often shifting from very warm to extreme cold, which frequently occasions inflammatory disorders among the inhabitants. Before the swamps were cleared and drained, the inhabitants used to be very subject to the *fever and ague*; but since, there have been no complaints of the kind in the town."

I hoped to have received some private information from this place, but my wishes have been disappointed.

¹ 1st series, IV, 15.

With regard to all the other towns designated upon the map, my evidence is merely negative, and I am unwilling to enlarge the sterile catalogue of names; which, in order to show that the ground had been traversed as thoroughly as was in my power, I have already rendered too tedious.

A few circumstances which were observed during the prevalence of spotted fever, appear to me worthy of mention.

Under the head "Anomalous Cases of the Epidemic," Dr. North remarks, "Anomalous cases have occurred, in which a paroxysm and intermission of the symptoms, like *ague and fever* have happened: some of these have continued during four or five weeks."¹

And in another place he says, "Fever is not a necessary attendant upon the disease, although some have a regular paroxysm of fever continue the usual term of ten, twelve, or twenty-four hours."²

In a memoir of an epidemic fever which prevailed in Wardsborough, Vt. (which was a petechial fever) by Dr. Allen,³ it is said, "If there were any regularity, it was in a greater number of exacerbations happening every afternoon; hence showing some small resemblance to the tertian⁴ type."

Dr. North, in the work above referred to, quotes some observations of Dr. Williamson, of Baltimore, on Anomalous and Irregular Diseases, from which the following remarks are extracted.

¹ Treatise on a Malignant Epidemic, &c. p. 17, (New York, 1811.)

² Ibid. p. 99.

³ N. E. Med. & Surg. Journ. Vol. IV.

⁴ The quotidian type.

"From the disease frequently putting on a periodical type, although it was not very well marked, yet I was induced to suspect it a species of *intermittent*; and I was also induced to believe those of the same nature whose prominent symptoms were analogous, even if they were less distinctly marked; and from there being little or no increased action in the pulse, from a perspiration succeeding when the pain either remitted or intermitted; and, finally, from the admirable success from the exhibition of bitters and tonics, I conceive my conclusion incontrovertible."¹

Dr. North remarks in a note, "This distemper, in Connecticut, would be called a light kind of spotted fever, and by physicians who have perused Senac's Treatise."

Such are the results which I have obtained, at the expense of no little time and labor, and which I offer as an answer to the question proposed by the Committee. Imperfectly as the facts may cover the vast area thrown open, I have at least the consciousness of having explored some neglected sources of knowledge, and of having rescued a few perishing traditions from forgetfulness. The difficulty of the task I have undertaken will hardly be realized excepting by such as may have made the same attempt; and if another should prove more successful than myself, I will not envy him the annual laurels, for they will seldom be more dearly purchased than by the wearied student who obtains them after the toils of this harassing investigation.

¹ Op cit. p. 185.

ON THE
NATURE AND TREATMENT OF
NEURALGIA.

“ Read not to contradict and confute, nor to believe and take for granted,
nor to find talk and discourse, but to weigh and consider.”

DISSERTATION.

“WHAT IS THE NATURE OF NEURALGIA, AND WHAT IS THE BEST MODE OF TREATING IT?”

BEFORE proceeding to the consideration of the two questions proposed, it is necessary to render as exact as we may be able, the term by which the disease to be discussed is designated.

The name Neuralgia, the Greek derivation of which need not be repeated for the hundredth time, is due to Chaussier, who thus successfully attempted to bring under a common title a certain number of painful affections, the type of which was to be found in Tic Douloureux and Sciatica. In itself it implies simply, *pain in a nerve*. But it was intended by Chaussier to be confined to certain kinds of pain in the nerves, particularly such as were subject to intermissions or exacerbations, and which followed the trunk or branches of the nerve affected through a greater or less extent of their anatomical distribution.¹ The term, however, has been used in a more extended sense by different succeeding authors. Thus M. Jolly (Dict. de Med. et de Chir. prat.) makes use of it to designate “all kinds of pain affecting the nervous fibre in general.” Mr. Teale uses it as “not being confined to those affections which are attended with pain, but also applied

¹ Dict. de Med. (1826), and Dict. de Med. et de Chir. prat. (1834).

to other morbid states of the functions of nerves,"¹ and consequently he treats of palpitations, of gastrodynia, and of flatulence, under the common head of Neuralgia. Dr. McCulloch, in his Tabular View,² divides the affection into *painful*, inflammatory, &c., and thus we find ophthalmia, diabetes and diarrhœa, under the same comprehensive title—Neuralgia.

With the aid of a little grammar, analogy, and common sense, it is not difficult to attach a more precise notion to the word in question. If we choose a classical form for the name given to a disease, it is for the sake of euphony and of uniformity in the nosology of different times and nations—not for the purpose of having a term which will accommodate the views of individuals in the teeth of all the linguists and lexicons whose confines we have invaded. Neuralgia then is and ought to be neither more nor less than *nerve-ache*, as cephalalgia is nothing but head-ache, and gastralgia nothing but stomach-ache. He who applies the word in question, to any thing but pain of a nerve, has the same right to apply the term cephalalgia, or its equivalent head-ache, to an affection in which there is no pain in the head. Even if the different affections, comprehended under the denomination Neuralgia, be owing to the same remote cause as this malady,—as, according to Dr. McCulloch, to malaria ;—if they have the same proximate cause and seat,—as irritation of the spinal cord or the ganglions in the view of Mr. Teale ;—still, there is no more reason for confounding them, than there would be for confounding cephalalgia and fracture of the skull, because both

¹ Treatise on Neuralgic Diseases, p. 42, note.

² Treatise on Remittent and Intermittent Diseases, &c. p. 471.

were produced by a blow upon the head; or cephalalgia and amaurosis, because both were the consequence of cerebral congestion.

On the other hand, if we confine the term to those cases in which the pain is intermittent, those in which it is not due to any obvious primitive disease, in short to the purer forms of *tic douloureux*, of sciatica, and of the corresponding pains of other nerves less commonly affected than those of the face and lower extremities, what classical term shall we apply to pains radiating through a nerve, in consequence of an injury or a tumor? Unless we choose to appropriate a new word to characterize these secondary pains, a word like *neurodyne* for instance, neuralgia is our only resource.

But as the perception and transmission of all pain of whatever kind, seems to be the province of the nervous system, neuralgia, or pain of the nerves, might be supposed applicable to any kind of pain. Yet the term evidently is intended to specify certain pains only; and how are we to draw the line of distinction? I believe this point may be rendered clear by a few physiological considerations. First, with regard to the nervous trunks and branches, we know that their healthy office is to transmit sensation, without sharing in it themselves. No man would ever conjecture the anatomical distribution of the trigemini or fifth pair from his healthy sensations. Even in the case of an injury of the face, in consequence of which the branches of these nerves transmit the sensation of pain to the organ of perception, the patient is totally ignorant in common cases, of the course of the channels through which the pain is conveyed. Such is the

history of simple pain, but such is not that of neuralgia of the nerves in question. In this affection they pass beyond their physiological limits of action, they become sentient instead of transmitting organs, and the finger of the patient can often trace as nicely as the scalpel of the anatomist, the delicate ramifications of nerves, whose existence is never suspected by the majority of mankind.

But with regard to the nervous expansion by means of which this system identifies itself, as it were, with the different organs, it is not so easy to show the distinction between common and neuralgic pain. Sharing in the effects of injury and disease with the surrounding parts, its natural function in these circumstances is pain, as much as increased circulation is that of the arteries in cases of local violence. Still, the general law which presides over this condition of the nervous expansion is, that the suffering shall be in proportion to the degree and nature of the injury or disease, its situation, and the individual sensibility of the patient; less in a trifling than a more severe wound, in a clean incision, than a violent laceration, answering in short to the final cause, which is informing us of the state of our organs. But if any part of the surface of the body, or of the internal parenchymatous tissues is the seat of pain wholly unaccounted for by its structural condition, violating the law of proportion between organic changes and sensation to which we have alluded, are we not justified in calling this pain neuralgia? I am disposed to answer affirmatively, and yet I would exercise caution in applying the term to particular cases, since we are constantly liable to overlook a real lesion by assuming too rea-

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dily that a functional disorder does not imply an organic change.

I am obliged, then, to consider neuralgia as the name of a symptom, and not of an essential disease always identical in its nature. I may add, that the more special term, *tic douloureux*, applies also only to a symptom, which, as we shall see, recognises many different causes, and calls for different methods of treatment. To conclude these introductory remarks, I propose the following definition of neuralgia. *Pain in the continuity of a nervous trunk or filament; or in its expanded extremities, when unattended by, or in obvious disproportion to, any organic changes in the part to which the pain is referred.*

ANTIQUITY OF NEURALGIA.

One of the first questions that arises is, whether Neuralgia be of modern origin or not. If it have always existed, then, like inflammation, it is independent of all those circumstances of manners and habits which vary from century to century. If, on the contrary, it first appeared in very recent days, then either the agents which affect mankind, or the constitutions of men have been modified. More than one writer has pretended that the people of modern days have been alone or peculiarly subject to this affection. "It appears" (says Hamel), "according to the opinion of a great number of learned men, who have thoroughly studied the ancients, that inflammatory and bilious affections were much more frequent, more simple and better characterized formerly than in our day, while in our own time, (*a fact*

apparently due in great part to the progress of civilization), catarrhal and nervous affections are much more frequently and extensively met with.”—“Some of these affections appear not to have been observed by the ancients, or at least they paid but little attention to them, since we find no trace of them in their writings. Such, among others, is that of which we treat, *a disease very common in our days*.”¹ In an Essay on Neuralgia, by Dr. Gorrie, of South Carolina,² the disease is supposed to be of modern origin, and remarkably on the increase. He speaks in the following terms. “Nor is it till we descend in the history of medicine in the latter part of the 17th century, that we discover any mark of its influence over mankind, and then it is so obscurely traced as to be with difficulty recognised.”

The majority of writers upon the subject have agreed that the first distinct and satisfactory notices of neuralgic diseases are to be found in some cases of *tic douloureux*, reported by André, a surgeon of Versailles, in 1756, in a treatise on the diseases of the *urethra*; in the work of Cotugno (*De Ischiade nervosa*, Neap. 1764), and in the paper of Fothergill on a Painful Affection of the Face (*Med. Obs. and Inquiries*, Lond. 1776). In the opinion of Monfalcon,³ neuralgia of the face was confounded by the ancients with *clavus hystericus*, *risus Sardonicus*, and especially with *odontalgia*; while *sciatica* was blended with *coxalgia*, *gout* and *rheumatism* under the general name, *pain of the haunch*.

¹ *De la Névralgie faciale*, par J. Ph. Hamel, Paris, 1803.

² *N. Y. Med. & Phys. Journal*, 1828.

³ *Dict. des Sc. Med. Art. Névralgie*.

A recent author¹ has attempted to show that the disease was repeatedly mentioned by the earlier classic writers. A case is quoted by him from Hippocrates, which he considers much nearer in character to neuralgia than many of the diseases described by the father of medicine, to those of which they have been supposed to be examples.

The history of this case is briefly as follows. Sense of flashing, like lightning, in the right eye, soon followed by pain about the right temple and through the whole head and neck. Tension (or swelling, according to Vanderlinden) towards the vertex; tension and hardness of the tendons; inability to move the head or open the jaws, on account of rigidity. The symptoms were relieved by emetics, venesection and hellebore. This case affords, in my opinion, but a very doubtful instance of the disease in question. It is perhaps with more justice that he speaks of Celsus as having been not unacquainted with the disease in question, relying upon the following passage found in connection with the history of the different species of head-ache. "*Præter hæc, dolor intolerabilis, maxime circa tempora, vel occipitium . . . hique omnes dolores modo in febre, modo sine hac sunt: modo in toto capite, MODO IN PARTE; INTERDUM SIC, UT ORIS QUOQUE PROXIMAM PARTEM EXCRUCIANT.*" (Lib. IV, Cap. 2.) Aretæus, as he remarks, and as is justified by his citations, is much more explicit and exact.

"*Formæ cephalæ infinitæ sunt. Quibusdam enim perpetuus dolor; . . . nonnullis per circuitus*

¹ Halliday, *Consid. prat. sur les Névralgies de la Face.* Paris, 1832.

revertitur, ut iis qui quotidiana intermittente febricitant dolor modo est in toto capite, modo in dextra magis, modo in sinistra, modo circa frontem, aut sinciput : hæcque eodem die incerto et erraticè fieri solent. Quidam dextra tantum parte dolent, quidam læva ; qua tempus, vel auris, vel supercilium unum, vel oculus ad medium usque terminatur, vel qua nasus in æquas partes dividit ; ultra quem terminum dolor non progreditur, dimidium tantum capitis occupans Haud leve malum : quamvis intermittit, quamvis exiguum esse prima specie videtur : nam si acute interdum impetum faciat, fœda atque atrocia detrimenta affert : nervi distenduntur, facies obtorquetur ; oculi vel contenti instar cornu rigidi sunt : vel huc atque illuc interius convelluntur, ac vertiginose agitantur : in ipsisque dolor profundus usque ad intimas tunicas descendit neque ulla causa præcessit, perinde ac si quis ligno plagam inflixerit (De caus. et sign. diut. morb. Lib. I, Cap. 2).

According to the same author (Halliday), the description of *raptus caninus*, a spasmodic affection of the face, given by Cælius Aurelianus, may perhaps be referred to the same disease, if this affection were indeed accompanied with pain, which he seems to infer from some passages found in other parts of this ancient author's writings. Neither Monfalcon nor Halliday, found any thing positively relating to neuralgia in the works of Galen, although the last gives one or two passages of doubtful import from his recollections of the Greek writer. We might certainly have looked for something relating to neuralgia in Galen, for in the index to the Junta edition of his works, more than sixteen crowded folio columns are occupied with *nervus* and its derivatives.

According to Halliday, the other Greek writers, the Arabians, and some still less ancient medical authorities, have nothing particularly bearing upon the subject of facial neuralgia. Monfalcon remarks, that the successors of Galen, the physicians of the fifteenth, sixteenth, and seventeenth centuries, never seized the true character of neuralgic diseases; and although they had met with several of them, as may be traced in their writings, that they did not distinguish them from diseases presenting certain similar appearances.¹

I believe this assertion of a celebrated writer, in a work famous throughout the scientific world, does injustice to a once-renowned, but now almost forgotten author. It will appear from the following quotation, that Fernelius had distinctly recognised sciatica as a neuralgic disease.

*"Ischias omnium vehementissima, sedem habet non in eo articulo, quo femoris caput in coxendicem inseritur, sed altius ad summam natem, qua nervi a lumbis et ab osse sacro emergentes feruntur in crura. Dolor atrox non in sola est coxendice, sed in femur quoque, et in suram, et in extremum pedem exporrigitur, quocumque nimirum ab affecta coxendice deductus nervus pertingit. Rarò tumor est conspicuus, rarius calor aut rubor, quod ejus loci summa cutis venis conspersa minime sit."*²

There is reason, then, to believe that the ancients were practically acquainted with neuralgia, however often they may have confounded it with affections having some of the same characters. The attention

¹ Dict. des Sciences Med. Art. Névralgie.

² Schenck. Obs. rar. p. 672. Lugduni, 1643.

of the physician was principally attracted by the disconnected complaints and contortions of the suffering patient; and how can we wonder that the anguish of *tic douloureux* was at one time confounded with headache, so frequently a distressing pain, and at another with toothache, the tortures of which are often sufficient to call forth all the eloquence of agony?

Within a few years, two diseases, often supposed not to have been previously noticed, have been brought forward; softening of the brain, by M. Rostan, and that form of *Paruria Inops* described by Sir Henry Hallford. Yet it would be easy to show, in a single volume, published nearly two centuries ago, that each of these diseases had been distinctly recognised. It is not to be imagined that no similar cases existed during this long interval, but only that they were overlooked or confounded with other affections. The same fortune appears to have attended neuralgia. When it is assumed then, as a fact, that this disease is of modern origin, it is in despite of many passages in the earlier writers which at least seem to have referred to it, and it is adopting a solution of the question which supposes a vast and widely-extended revolution in the conditions of the human body, instead of an imperfection in analytical observation, which was natural and necessary, and which we meet with at every step in medical history. For us, then, neuralgia is not a disease of modern origin, and we are spared the discussions sometimes indulged in upon the ruinous effects of advancing civilization and refinement, the enervating influences of tea and coffee, and similar matters of speculation.

Neuralgia, then, being an affection so far as we know incident to the human race at large, we are next to inquire under what conditions of the suffering part, or of the system in general, it appears.

So long as we are ignorant of the mechanism by which the functions of the nerves are accomplished, we cannot hope to comprehend the ultimate nature of their deranged action. Were it certain that the nerves, like the arteries, were hollow tubes, conveying a fluid, we might look for a cause in the derangement of the nervous circulation. Were this fluid identical with electricity or galvanism, we might inquire if any changes had taken place rendering the nerve diseased less capable of conducting it. We might conceive that if an uninterrupted chain of communication between the nervous extremity and the central organ were the physiological condition, an entire interruption of this chain in a sentient nerve would produce paralysis of sensation, and a partial interruption, different modifications of sensation; at one time formication, at another pain, or neuralgia; in the same way as the electric fluid which passes silently through an unbroken wire, becomes accumulated, and leaps violently from point to point when the chain of transmission is broken. Were the nerves vibrating or undulating cords, any mechanical derangement which should render the vibrations or undulations irregular, might be supposed capable of affecting in different modes the perceptions resulting from their movements. But ignorant as we are, and probably ever must be, of the manner in which the will of the individual circulates through hundreds of muscles by means of a few slender filaments; or the impressions of

the outward universe through a few bundles of medullary fibres, it would be frivolous to attempt a discussion of a point which implies this previous knowledge. It is enough to discuss those phenomena which are matters of positive observation.

The different authors who have written upon neuralgic affections have come to very opposite conclusions upon their proximate cause. I shall first consider the opinions of those who place it in some alteration of the nerve affected.

In the single case examined by Cotugno, which was that of a patient affected with sciatica, a considerable serous effusion was found within the sheath of the nerve at its lower part, to which condition he was inclined to attribute the neuralgic affection in this and other cases.¹ But this patient was a dropsical person, and Cotugno had no other evidence in favor of his hypothesis than the success which he professed to have obtained from the treatment founded upon this basis. An œdematous condition of the parts surrounding the nerve has been sometimes observed by others, but there is reason to consider this as a secondary lesion, and the opinion of Cotugno has hardly found an advocate.

Among the probable causes of neuralgia, Mr. Ley admits "vascular congestion and irritation of the nerve itself."² There is, perhaps, no positive evidence to be found in proof of this supposition. "The nerves are a little more reddish," says Bichat, "in some cases

¹ Monfalcon, *Dict. des Sc. Med. Art. Névralgie*.

² On the Pathology of Nerves. *Lond. Med. Gaz.* Jan. 3d, 1835, *et seq.*

than in others. Do these instances coincide with any determinate morbid affection? I have no light, as yet, upon this point."¹ Mr. Ley admits the difficulty of drawing the line between congestion and inflammation, and founds his argument rather upon analogy than observation. Neither of the cases cited as countenancing this supposition is, in any degree, convincing; for in the case of sciatica mentioned by Bichat, which is considered by Mr. Ley as the most satisfactory, the veins of the nerve were found in a *varicose*, or permanently dilated state, which may indeed be an effect of congestion, but is evidently a structural lesion, and to be distinguished from the condition by which it may have been caused.

A more generally received opinion is, that neuralgia is due to inflammation of the nerve, or of the neurilemma. This hypothesis is attributed by Cooper,² by Dr. Gregory,³ and by Dr. Gorrie,⁴ to Dr. Parry, of Bath. The elements of Pathology and of Therapeutics of the latter author, however, were published in 1815, but the notion that the proximate cause of neuralgia consists in inflammation of the nerve was distinctly brought forward in the treatise of Hamel, published in 1803.

Among the authors who have countenanced this opinion are Broussais,⁵ Monfalcon,⁶ and Piorry.⁷ I have called this explanation an hypothesis, because it is by no means warranted as a general conclusion by the results of pathological anatomy, which in many cases

¹ Anat. Gen. I. 155.

² Surg. Dict. Art. Tic Doul.

³ Practice of Physic.

⁴ Dissert. Cit.

⁵ l'x. des. Doct. Med. p. XLVII.

⁶ Dict. des. Sciences Med.

⁷ Clin. Med. de l'Hop. de la Pitié, Paris, 1835.

must be the test of the condition of parts. As, however, it has found powerful supporters, as it is one of the explanatory ideas which the mind most readily adopts, and as it has a manifest bearing upon the indications of treatment, it deserves a serious discussion.

I shall examine this point, first in the light of pathological anatomy; secondly, by comparing the symptoms of the disease, and the effects of treatment, with the same circumstances as they appear in inflammation, and thirdly by glancing at the history of true inflammation of the nerves, or neuritis. I may begin by citing the authority of Andral. "In many cases," he remarks, "in which, during life, the seat of the disease had evidently been in the nerves, investigation after death has not revealed any appreciable lesion. I have frequently examined the nerves in cases of sciatica, both recent and of long standing; but I have never been able to find the slightest alteration, unless in a single case, where the nervous trunk, which had been the seat of pain during life, was manifestly injected. In the case of a woman, who, during the latter months of her life, had constantly suffered from very acute pains in the left lateral and posterior parts of the neck, and about the occipital bone, which pains had all the characters of neuralgia, I dissected with the utmost attention the nerves of the brachial and cervical plexus, without discovering any thing morbid."¹

Even Monfalcon agrees that, "the different dissections of nerves which had been the seat of neuralgia, teach us little or nothing"—and he adds, that "the most common phenomena which have been observed,

¹ *Trait. d'Anat. Path.* Vol. II. p. 854.

are the increased size of the nerve, sometimes due entirely to the œdematous state of the cellular tissue, and the more or less voluminous varices of its veins.”¹

Sir Astley Cooper declares, that in tic douloureux, the nerves are certainly not in an inflamed state, for, under the most horrid suffering, they are found of a natural color: that they are diminished rather than increased in size, and he considers the disease one of diminished rather than increased action.²

Mr. Lawrence considers certain cases of sciatica due to inflammation of the nerve; remarking that we sometimes find the nervous cord surrounded by thickened, indurated cellular membrane, and the nerve, as it were, expanded, doubtless from effusion into the cellular tissue. But, on the other hand, in speaking of tic douloureux, he declares that we often find nothing to account for the sufferings of the patient. He speaks of the case of a gentleman who had been subject to tic douloureux for a great many years, in which upon dissection, after death, he could find no disease of the nerve,—which, however, he did not follow through the bony canals.³

Mr. Brodie gives a case in which the nerves of the fifth pair were found healthy in appearance, on the post mortem examination of a gentleman who had suffered from tic douloureux, followed by epilepsy, ptosis on the affected side, and fatal apoplexy. In this case the affection seemed to have been due to disease of the brain and its membranes.⁴

¹ Dict. des Sc. Med.

² Lect. on Princ. and Pract. of Surgery. London, 1832, p. 437.

³ Lect. on Surgery. Lond. Med. Gaz. Vol. VI. p. 643.

⁴ On Tic. Doul. Lond. Med. Gaz. Vol. 17th, p. 534.

In the opinion of Halliday, "the cases which so frequently happen, in which tic douloureux has been known to last fifteen or twenty years without causing the formation of any appreciable organic lesion, ought to induce the belief, that there is in reality nothing like inflammation."¹

M. Jolly testifies to the same effect that the results of dissection have frequently afforded no sign of disease.²

But perhaps the strongest authority upon this point is M. Martinet, who, as he declares, in his celebrated paper upon neuritis (névrite) had been engaged in studying the affections of the nervous tissue for nearly ten years, and who has himself brought together more instances of inflammation of the nerves than any other author with whom I am acquainted. "For our own part we believe that neuralgia *almost always* depends upon an irritation fixed on the medullary substance of the nerves, but an irritation which has no tendency to change into inflammation; that in these cases they present certain signs which are entirely characteristic; at other times, on the contrary, and *in the smaller number of cases*, they are owing to inflammation of the neurilema, and are distinguished from the first by certain peculiar characters."³

M. Piorry has attempted, however, to meet the negative evidence furnished by pathological anatomy. The following is the substance of his arguments.

1. The fact of not finding any appreciable lesion after death, in the nerves, does not prove that they

¹ Op. cit. p. 119.

² Dict. de Med. et Chir. Prat.

³ Memoire sur l'Inflam. des Nerfs. Rev. Medicale, 1824.

have not been the seat of irritation, and even hyperemia, for, in cases of pharyngitis observed a little before death at La Salpêtrière and La Pitié, no traces of the disease were found on the body. *A fortiori*, this might be true with respect to the nerves.

2. The diseased process may attack single filaments too minute for dissection. How then can we judge of those shades of color in their nervous pulp which are considered as the characters of inflammation in the brain?

3. The vessels which supply the nervous filaments are so minute that they must be greatly enlarged to render their hyperemia appreciable.¹

In reply to the first argument, it may be remarked that although the blood often deserts the mucous membranes after death, yet this is not true of the muscles, and may not be of the nerves. Besides, it is proved by dissection that the nerves in certain cases do retain their increased redness after death,² and therefore when they present no such redness there is some reason to suppose it never existed.

As to the second argument, that the disease may attack single filaments, too minute for dissection, it is by no means countenanced by the history of the disease; for the pain usually radiates from some point corresponding to a nervous trunk, and not through one, but through many of the branches proceeding from it, as may be particularly seen in the affections of the fifth pair, and those cases of *tic douloureux* which have been supposed owing to an affection of the *portio dura*.

¹ Clin. Med. de l'Hop. de la Pitié, p. 283-84.

² Hamel, p. 19. Martinet, Obs. 4th, 5th, 6th, 9th, 10th.

To the third argument of M. Piorry, it may be replied, that the vasa vasorum, the vessels of the conjunctiva, and the cartilages, which usually carry a colorless fluid only, minute as they are, become readily appreciable when in a state of inflammation.

We are next to compare the symptoms of the disease with those of inflammation.

First, the common signs of inflammation, redness, heat and swelling, are frequently, if not generally, wanting in neuralgia, or if they occur during a painful paroxysm, they are obviously an effect of the disturbance of the nervous function, which, after a certain time, increases the circulation and secretion in the parts where it is manifested.

Secondly, the pain resulting from inflammation is usually gradual in its approach and disappearance. That of neuralgia, on the contrary, frequently begins and ceases with the suddenness and abruptness of an electric shock.

Thirdly, true inflammation rarely, if ever, assumes the periodical type. Some instances of affections having inflammatory characters, and returning at fixed intervals, have indeed been collected by Casimir Medicus, but they form singular exceptions only to the common law. Even in the very limited number of these instances, he has included two cases which seem to resemble neuralgia more nearly than inflammation.¹ Allowing to these facts whatever value they may possess, they do not in the least invalidate the general rule, that inflammations in the infinitely greater proportion of instances do not affect the periodical form.

¹ *Traité des Mal. Period. sans Fièvre*, (Paris, 1790).

But in neuralgia, on the contrary, this form is so common as to present one of the most frequent and satisfactory indications of treatment, and to have given to bark and arsenic the character of specific remedies for a large class of cases.

Again, the facility with which neuralgic pains are brought on by the slightest causes, and carried at once to their highest intensity, presents a strong analogy to other phenomena undoubtedly due to the nervous action purely, and on the other hand is remote from the phenomena of inflammation. Thus the convulsive action of the diaphragm, produced by irritating the fauces, the muscular tremor caused by a sudden emotion, the peculiar sensation about the præcordia, accompanying certain kinds of apprehension, like that, for instance, which every boy has remarked in swinging; or complete syncope, or convulsions from moral causes; all these are instances of the rapidity with which the nervous influence is propagated in the case of unusual impressions, as the common phenomena of sensation and movement are proofs of the same rapid transmission of that influence under the ordinary stimuli. And in the same way, when, as I have known in one instance, an acute pain suddenly appears in the posterior part of the thigh, almost at the instant that the feet have touched a cold floor, or at the moment when a door has been opened, or upon the contact of cold sheets at night; or, as has occurred in two cases within my own observation, when the pain has habitually appeared at the approach of a thunder-shower; or even upon directing the attention to the subject, as in a case related by Dr. Fothergill, it is natural to suppose that the

sensation is produced in consequence of some direct modification of the nervous agency, and not through the tardy machinery of inflammation. But it may be said that these trifling agents produce the painful paroxysm by some additional impulse they communicate to the nerve already in a state of chronic inflammation, in other words, that the inflamed nerve may at one instant be totally free from pain, and the next instant, merely in consequence of such an impression as the nerves are constantly in the habit of receiving, may become the seat of the greatest agony to which the system is subjected. Now if this suffering can be conceived due to the union of an insignificant nervous impression, like that from a cold floor, or a gust of air, and of an organic condition unaccompanied by pain the moment before ; if an infinitesimal exciting cause, in conjunction with a lesion in itself incapable of affecting the sensation, can produce such a degree of pain, does not this suppose a profound change of the nervous susceptibility in addition to these apparent elements ; and if such a change do really exist, is this supposed inflammation, inappreciable during life, or after death, capable of accounting for it ?

It is worthy of note that the painful paroxysm is not equally induced by any impression upon the nervous extremities ; nor is it in proportion to the amount of common sensation which the impression produces ; on the contrary, a breath of cool air, or an electrical change in the atmosphere, which the healthy nerves would scarcely recognise, are sufficient to bring on a violent attack, which, again, is relieved in many cases by the application of more energetic stimuli, as heat and friction. It is evident that even if the supposi-

tion of inflammation could explain an increased susceptibility of the nerve to stimuli, yet that it cannot account for this *elective* susceptibility, which implies not merely an increase, but a perversion of the functional properties of the nerve; therein differing from the known effect of inflammation in other tissues, which is, that the inflamed part suffers under the application of all stimulants indiscriminately, and in proportion to the force which they possess of exciting the healthy sensibility in the part affected.

Once more, the character of the pain implies something different from inflammation. It may throw some light on the subject to compare the pains of neuralgia with the phenomena of spasmodic action, which they resemble in certain points, and with which they are not unfrequently connected. Both are for the most part interrupted by intervals, of ease in the one case, of repose in the other. We can explain this to a certain extent in spasm in the following manner. The influence by means of which muscular contraction takes place, is transmitted through the nerves not continuously, but in successive jets or impulses, rapid and uniform in robust individuals, and in an undisturbed state, less uniform and probably slower in those who are feeble, after fatigue, or under depressing emotions. This has been illustrated by that which happens in holding a weight with the arm extended. At first there is only slight tremor, but as the energy becomes exhausted, the muscular starts become more and more obvious, showing that these pulses of the nervous *current* (to use a scholastic phrase) are less equable in their rhythm or force than at first. But if muscular contraction be owing

to a series of nervous stimulations, we can readily conceive that one or more of the series may be in excess, and that this excessive stimulation may be followed by one or more nervous impulsions of the natural or less than the natural degree of intensity; thus producing spasm followed by repose, which is only an exaggeration of the phenomena of muscular tremor.

There is every reason to suppose that the action of the nerves of the senses is made up in the same way of a series of distinct changes or impulses transmitted through their substance. This must be true of the nerves of hearing, for every sound, however apparently continuous, is composed of a definite number of undulations. The experiment mentioned in all the treatises on Optics, of the disk painted with the seven primary colors, which appears white when in rapid rotation, serves to show that in the same manner a single visual perception results from a number of distinct impressions. It is not so easy to prove that the same law is true of the nerves of common sensation; but every analogy leads us to suppose that the function of these nerves consists of a succession of distinct impulses or actions (for I use these words as the least objectionable), traversing their substance. Let us now suppose the case of a fit of neuralgic pain, brought on by the application of cold. We will suppose that the nerve has conveyed to the sensorium ten impressions from the surface, which have produced one single and continuous perception—cold. But suppose the eleventh impression to be irregularly transmitted by the affected nerve; pain may evidently be the result, and this will continue just as

long as the successive transmissions are irregular; being a single twinge if only one is deranged, a continuous pain if many are so, or felt at intervals if a certain number of regular impulses intervene between the irregular. This is precisely parallel to that which happens in muscular spasm, and may be still farther illustrated by what takes place in certain forms of palpitation, in which the heart, after performing its actions regularly a certain number of times, suddenly receives an extraordinary stimulus, and acts with violence and irregularity, subsiding immediately afterwards into its ordinary rhythm and force. When it is remembered that the structure of the nerves of motion and sensation is exactly similar, so far as we can detect, the latter being distinguished only by their superadded ganglia, we have still farther reason to suppose their manner of action similar, both in health and in disease. If the analogy I have spoken of be just, neuralgia is to the one what spasm is to the other. If we do not meet with any cases of general neuralgia, as we do of general spasmodic action of the muscles under control of the voluntary nerves, this is easily accounted for by the fact, that in one of these systems the stimulus exists at the centre, and in the other that the sources of excitement are at the circumference. The brain and spinal marrow are, as it were, a reservoir to which the sensations converge, and from which the principle of motion is poured through diverging channels. A change in these central organs may readily affect all the channels through which their influence is distributed, while it is difficult to conceive how there can be a simultaneous derangement in the functions of many nerves whose branches

are independent of each other, whose stimuli are external, and often entirely distinct in their nature.

If I have succeeded in establishing the analogy spoken of, the nature of neuralgia may be illustrated by that of spasm. Now it is perfectly clear that spasm is not inflammation of the nerves of motion. The supposition is even absurd to the highest degree. We should be obliged to attribute tetanus, hysteria, epilepsy, chorea to a single proximate cause. Inflammation of the nerves must be almost always fatal (tetanus) and almost never fatal (hysteria); it must resist all kinds of treatment, and be instantly calmed by a cup of Valerian tea, or a few grains of assafœtida; vomiting must be inflammation of the nerves of the stomach, and hiccough inflammation of the nerves of the diaphragm. It seems probable from these considerations that neuralgia, like spasm, does not essentially consist in inflammation of the nerves.

Nor do the effects of treatment countenance the hypothesis that the affection is essentially inflammatory. In the numerous cases which have been recorded, we are constantly meeting with the fact that depletion and counter-irritation are often entirely unavailing, and that in every variety of the affection. Thus, in some of the cases mentioned by Halliday, both leeching and blistering were applied in neuralgia, affecting the cerebral nerves, either without benefit or with positive injurious effects. So in the neuralgic affection of the breast, Mr. Lawrence declares that leeching is of no utility;¹ as an instance of which, the case related by Dr. Good,² in which

¹ Lond. Med. Gaz. Vol. XVI, p. 715.

² Study of Med. Vol. IV, p. 222.

"bleeding, local and general, frequently and profusely repeated" was of no avail; or a similar one to be found in the *Gazette Médicale de Paris*,¹ may be referred to. And so in sciatica, which Mr. Lawrence² considers, perhaps justly, as being more frequently inflammatory than some of the other forms of neuralgia, and in which depletion and counter-irritation are certainly often useful, they may fail altogether. Almost every practitioner has had the opportunity of learning this; but I may simply advert to the instance of Mr. Skey, who, in his own case, tells us that he had thirty-six ounces of blood drawn by cupping over the sciatic nerve, besides using blisters, liniments and embrocations, all without relief.³ That they do not succeed in neuralgia testis, may be inferred from Sir Astley Cooper's having performed amputation of this organ no less than three times for this distressing complaint.⁴

It remains to examine the phenomena which occur in well marked cases of genuine neuritis, or inflammation of the nerves. And, first, it is not true that neuritis is always accompanied by neuralgia. If it were so, we should have the ferocious pains of the latter morbid affection after every amputation. Nay, if it were true, according to the doctrine of M. Piorry, that neuralgia may be due to an inflammation of filaments so minute as to escape anatomical appreciation, every considerable wound and contusion would be liable to be speedily accompanied by neuralgic symptoms, for these filaments must participate in the inflammation of the injured parts.

¹ For May, 1836.

² Lond. Med. Gaz. Vol. VI, p. 643.

³ Ibid. Nov. 5, 1836.

⁴ Ibid. Vol. VI, p. 718.

It appears, however, from the most satisfactory evidence, that the painful affection of which we are treating, is sometimes a consequence of inflammation of the nerves; just in the same way as headache is sometimes, though by no means generally produced by inflammation of the brain, or its immediate envelope. Neuritis is sometimes the consequence of mechanical injuries. If neuralgic pains accompany the inflammatory consequences of the injury, they may be presumed due to the inflamed condition of the nerve or surrounding parts. If, on the contrary, they appear after the cessation of the inflammatory signs, it is perfectly conceivable that they may be owing to some change of structure in the nerve, which there is no more need of supposing inflammatory, than there is of confounding a cicatrix with a phlegmon; and we might in fact as well apply leeches to the cicatrix, with the hope of removing it, as to some of these structural changes. Instances have occasionally happened in which neuralgia has followed soon after the laceration of a nerve. Such a case may be found in one of the numbers of the London Medical Gazette.¹

The patient, a coachman, aged forty-five, fell seven or eight yards upon a spiked railing. Two wounds were inflicted, one an inch in extent in each direction, and triangular, in the back part of the right thigh, about three inches below the tuberosity of the ischium, the other at the outside of the anus. A number of lacerated filaments of the sciatic nerve were hanging from the wound in the thigh, some of which, three inches and a half in length, were cut away. Four

¹ Vol. X, (1832). Case communicated by J. M. Banner, of Liverpool.

hours afterwards there were pains shooting to the loins and down the leg to the foot, with occasional spasmodic twitchings. These had been preceded by numbness and pain in the thigh. On the second day there was great pain; the spasms and numbness continued. There was twitching of the hands and face during sleep. After abating on the third day, the pains in the leg returned on the evening of the fourth, extending to the foot; affecting the origin of the toes, and very acute at the edges of the foot; they also radiated from the wound to the lower part of the back. On the ninth day the foot was acutely sensible when touched lightly; otherwise not painful. After this he improved; resumed his duties forty days from the time of the injury, and a fortnight later was gaining a more perfect use of the movements of the leg, and the sensibility of the heel, being in other respects quite well.

Among the cases reported by Mr. Swan,¹ are several in which the neuralgic pains followed soon after different kinds of local violence. Thus, in one case, they commenced shortly after a violent blow,² in another they seem to have commenced soon after a fracture of the femur,³ in others they followed venesection⁴ at a short interval from the operation. Of twenty-seven cases of neuralgia given in the work of Halliday, the affection followed an injury in two, bleeding from the temporal artery in the one,⁵ and a blow upon the frontal bone in the other;⁶ but it does

¹ Dissertation on the treatment of morbid local affections of Nerves. London, 1820.

² Page 46.

³ P. 101.

⁴ Pp. 107, 116.

⁵ Obs. 21.

⁶ Obs. 27.

not appear that in either of these cases the painful affection was the immediate consequence of the inflammatory accidents. In one, at least, of the cases given by M. Piorry,¹ neuralgic pains followed within a few days after a contusion.

But it is much more common to find neuralgia declare itself at a considerable distance of time from the injury which is its apparent cause. Were it due strictly to inflammation, however, we should look for it in the inflammatory stage of the injury. To give examples in support of the assertion just made, the pains felt in old cicatrices, during or previously to changes of weather, are by no means in a direct ratio to the inflammation caused by the original lesion; on the contrary, they appear when every trace of this condition has vanished. And in the neuralgic pains following amputation, it is not while the extremities of the nerves, like the surrounding tissues, are inflamed or actually suppurating, that we have this symptom, but when these active processes have subsided. And this form of neuralgia, it is to be remembered, is one of the most common as well as the most violent; so much so as to have drawn the attention of almost every surgical writer since the publication of Mr. Langstaff's paper in the *Medico-Chirurgical Transactions*, and to have supplied the journals with some of the most painful medical histories on record.²

In the memoir of Martinet to which we have before referred, ten cases are given, several of which were

¹ Case 4.

² Vide *Lancet*, Oct. 8, 1836. *Lond. Med. Gaz.* Vol. XIII, p. 782, and Vol. X, p. 495.

communicated to the author by M. Recamier. The three first of these cases were observed only during life, the seven others are accompanied by an account of the post mortem appearances. Each of the three first cases presented an increase in the size of the nerve appreciable to the touch, and in two of them the pain was augmented by pressure. The examination of the others after death, revealed either redness, hypertrophy, or purulent infiltration of the affected nerves. From these cases of neuritis, M. Martinet derives the following conclusions.

1. That the inflammation of the nerves is one of the causes of neuralgia, but one of those most rarely observed.

2. That this inflammation is seated in the neurilema and cellular tissue uniting the nervous filaments.

3. That this inflammation offers *as a constant character*, the development and increase of the pain by pressure upon one of the points of the inflamed nerve.

4. That this inflammation differs from essential neuralgia, which consists in the simple change of the sensibility of the medullary substance, by the circumstance that the latter is not always exasperated by pressure, that it is excessively variable by its nature, and that it is always accompanied with remissions.

I finish these remarks upon the hypothesis of inflammation being the cause of neuralgia, by the following conclusions.

First—As M. Martinet has declared, and as his cases demonstrate, inflammation of the nerves is one of the causes of neuralgia.

Secondly—Pathological Anatomy, the history of the symptoms, and the effects of treatment concur in

showing that the greater number of cases of the affection we are considering, are owing to some condition different from inflammation.

Another theory, which, according to M. Jolly, has been advocated by Cabanis and by Cuvier, and which is brought forward in the recent work of Roche and Sanson,¹ is, that the different *neuroses*, among which M. Roche comprehends neuralgia, depend on "the accumulation of the *nervous fluid* in a tissue." The existence of a nervous fluid is of course a matter of hypothesis. The supposition of such a fluid reconciles itself with many of the nervous phenomena, and certainly is as rational as many of the conjectures which we meet with in physiology. But however well it may be applied to explain the actions of the nervous system in its natural condition, however it may aid us in accounting for some of the characters of neuralgia, the question still remains, Why does the fluid accumulate in this or that nerve; in other words what is it that deranges the flow of this nervous fluid; what is the cause of the disease? Assuming this explanation as correct so far as it goes, it leaves us exactly where we began, as to the real source of the painful affection, applying itself equally to the different hypotheses and facts which we have seen or are to examine in succession.

Among the local causes upon which neuralgia depends, there is one which cannot be called in question. This is the mechanical derangement of the nervous tissue, produced by accidental or persisting

¹ Nouveaux Éléments de Pathol. Méd. Chirurg. Paris, 1833, Vol. II, p. 278.

causes. One of the simplest forms is that in which the nerves of the pelvis are compressed by the head of the fœtus during parturition. Very severe cases of neuralgia have been caused by the presence of tumors in the immediate neighborhood, or sometimes even in the substance of a nerve. A foreign body lodged in the flesh near a nerve, or in its substance, may produce effects similar to those of a tumor, since, during muscular action at least, it must exercise pressure on the neighboring parts. A well known case of this kind is that related by Dr. Denmark, in the *Medico Chirurgical Transactions*, and frequently referred to by other writers, in which a piece of a musket ball was imbedded in the median nerve, and where the symptoms were so violent as to require amputation. M. Piorry refers to a case in which neuralgia of the external popliteal nerve followed a bullet wound at the outer part of the leg.¹ A similar instance is the one related by Jeffreys, and referred to by Good, Elliotson, Abercrombie and others, in which a piece of a tea-cup, imbedded in the cheek, afflicted the patient for fourteen years with *tic douloureux*, and was at last extracted by an incision, after which the sufferings ceased. Tumors in the nerves, according to Mr. Lawrence,² are rare; he had never seen them except in anatomical collections. He exhibited to his class a tumor of the popliteal nerve, of the size of an orange. According to M. Andral,³ the nerves have been found the seat of encephaloid, tuberculous, schirrous, and

¹ *Op. cit.* p. 283

² *Lect. in Lond. Med. Gaz.* Vol. VI, p. 643.

³ *Anat. Path.* Vol. II, p. 888.

other tumors, among which are cysts, containing gelatinous, fibrous, or cartilaginous substances. Sir Charles Bell has mentioned in his *Operative Surgery* a case in which the patient suffered from violent pain in the foot for two years, and upon examination after death the sciatic nerve was found entering into the substance of a tumor in the ham.¹ In the lecture just referred to in the note, he speaks of it as follows. "The worst case of this kind I ever met with, was where a small tumor invested the popliteal nerve; the man actually died of pain—pain in the sole of the foot." The painful subcutaneous tubercle, described by Cheselden, Wood, and others, and of which the most complete account is to be found in the work of Descot,² offers perhaps the most frequent examples of neuralgia from tumors. Mr. Lawrence refers to two cases in which pain of the most violent kind, in the arm, forearm and hand, was the consequence of the development of aneurisms in the midst of the axillary plexus, by which some of the nerves were flattened out like tape.³

Another mechanical cause, similar in effect to the pressure exercised by tumors, is the tension which some of the fibres of a nerve undergo when it has been partially divided.

To show, says Mr. Swan, "that the wound of a nerve may be the entire and immediate cause of the symptoms, independent of inflammation, or any thing

¹ Lond. Med. Gaz. Vol. XIII, p. 765, and Swan's *Dissertation*, p. 79, &c.

² *Dissert. sur les affect. loc. des nerfs.* Paris, 1825.

³ Lond. Med. Gaz. Vol. VI, p. 643.

else that could irritate the nerve, the following case of wounded nerve from bleeding in the foot, related by Sabatier, will prove, as in this instance, they must have been owing to the peculiar form of the wound."

"‘This slight operation,’ he says, ‘was very painful, and was soon followed by convulsive motions, which extended themselves through the whole of the wounded extremity, and then through the rest of the body: these symptoms were not accompanied by any tumefaction, and were very often renewed. The patient could neither walk, nor ride in a carriage. This state having continued a long time, notwithstanding the use of anti-spasmodics and quieting remedies, I advised a division of the saphenus nerve, but it was not consented to; nevertheless the nervous symptoms gradually diminished, and the patient partly recovered her health, after five or six years almost continual suffering.’”¹

It is not very clear how far neuralgic pains attended this affection, but the case is useful as illustrating the effects of mechanical injury of a nerve.

I cannot resist the temptation of translating a few paragraphs from that quaint and delicious writer, the surgeon of princes, and the prince of surgeons, whose grave and imposing bust still holds the central pedestal of the Parisian École de Medecine. It is an interesting case, and has not, to my knowledge, crept into our common manuals.

“HISTORY OF THE DEFUNCT KING, CHARLES IX.”

“Now, to instruct the young chirurgien, and the

¹ *Diagn. cit.* p. 113-14.

better to prepare him for the above-mentioned practice, I will relate this history, which is not out of place with regard to the cure of punctures of nerves. The king having a fever, Monsieur Chapelain, his first physician, and Monsieur Castelan, also physician of his Majesty, and first physician to the queen his mother, ordered him to be bled ; and to do it they called one that had the name of a skilful bleeder, who in attempting to open the vein, punctured the nerve ; which made the king cry out at once, saying that he had felt a very great pain. Wherefore I ordered somewhat peremptorily that they should loosen the ligature, otherwise the arm would swell up greatly, which in fact soon happened, with a contraction of the arm, so that he could not bend or extend it freely, with extreme pain both at the place of the wound and throughout the whole arm. As the first and readiest remedy, I applied a little plaster of basilicon, for fear the wound should unite, and compresses steeped in oxycrat over the whole arm, with an expulsive bandage, beginning at the wrist and finishing near the shoulder, to cause a reflux of the blood and spirits to the centre of the body, for fear the muscles should suffer from too great fluxion, inflammation, and other accidents. This being done, we retired apart to advise and conclude what remedies should be applied to calm the pain and obviate the accidents commonly following the puncture of nerves. I submitted the proposition that it was proper to treat the puncture with oil of turpentine, made somewhat warm, and with a little rectified brandy, and to apply over the whole arm a plaster of *diachalciteos*, dissolved with vinegar and oil of roses, continuing in the mean

time the bandage aforesaid. My reasons were, that the said oil and brandy have the power of penetrating to the bottom of the puncture and drying the moisture which came out of the substance of the nerve; as well as of assuaging the pain both by their actual and potential heat: and the said plaster of diachalciteos has equally the virtue of resolving the humor already arrived in the arm, and prevents the descent of fresh humor. To the which the aforesaid physicians agreed, and concluded that such remedies were useful and necessary. By these means the pain ceased. And in order still further to resolve and drain the humor contained in the part, resolute and desiccative remedies were employed, like the following:

R. Farinae Hordei et Orobi ana ℥ij.
 Flor. Camom. et Meliloti ana p.ij.
 Butyri recentis sine sale ℥jss.
 Lixivii barbitonsoris q. suff. ad formam pultis.

“The king was three months and more without being able to bend or extend the arm freely: nevertheless (blessed be God) he was perfectly cured, with no lasting injury of the movements. Now we had concluded, if the above remedies had not proved sufficient for the cure, to make use of boiling oil, in order to cauterize the nerve, or even to divide it completely: because it was better that he should lose the use of the arm than that he should perish miserably for want of this operation.”¹

I will revert for a moment to the subject of com-

¹ Oeuvres d'Ambroise Paré. Paris, 1579, p. 401.

pression exercised upon nerves, to mention a singular case quoted from Portal, by Mr. Swan.¹

“The subject was a woman who had a very great curvature of the spine, and three or four hours after each meal complained of much pain in the great toe of the left foot: it was always increased by injections, but went off when she had a copious alvine evacuation. It was found to have been produced by pressure made by the last false ribs on the sigmoid flexure of the colon, which caused the fæces to have great difficulty in passing, and in consequence compressed the lumbar plexus of nerves.” In the 18th case of M. Piorry the affection was clearly due to stercoral concretions.

Of the neuralgic pains following amputation, as we have before remarked, we have too frequent examples. It is well known that the extremities of the divided nerves become enlarged and offer a bulbous appearance when examined at a certain period after the operation. According to Mr. Liston, this change takes place soon after amputation, and results from the exudation of a grayish matter into the cellular tissue uniting the nervous filaments. He attributes the pains, of which they are often the seat, to their being too exposed, and not sufficiently protected by the soft parts, or to their being stretched and strained by adhesions.² In a case referred to, the extremity of the sciatic nerve was found cartilaginous after a second amputation, and the operation being again required by the continuance of the pains, it was found

¹ Diss. cit. p. 145.

² Lancet, April 3d, 1836.

enlarged, but not cartilaginous.¹ Mr. Mayo considers the neuralgic affection accounted for by the contraction of the muscles left attached to the end of the bone; the nerves being more than naturally sensible on account of their enlarged extremities.²

Mr. Lawrence considers it doubtful whether the pains arise from the connection of the extremities of the nerves with the cicatrix, or from their being pressed by its contraction against the end of the bone, but he remarks that the pain is in many instances referred to some particular spot at the end of the stump, indicating the affection of some nerve connected with a certain portion of the cicatrized parts.³ Whatever be its cause, it persists long after the inflammatory symptoms following the operation, and is cured by removing the end of the stump, or at least of the nerves—in other words, by a process necessarily followed by active inflammation and suppuration in the majority of cases.

In connection with the local causes of neuralgia, I may refer to the opinion of Sir Henry Hallford, founded on his experience, “that the disease (*tic douloureux*) is connected with some preternatural growth of bone, or a deposition of bone in a part of the animal economy where it is not usually found in a sound and healthy condition of it, or with a diseased bone.” Sir Henry mentions four cases of *tic douloureux*, of which the following is an abstract.

1. That of a lady, aged forty. An exostosis had

¹ Case of Anna Allen, *Lancet*, Oct. 8, 1836.

² *Ibid.*

³ *Lect. in Lond. Med. Gaz.* Vol. VI, p. 643.

formed at the root of a tooth, and an almost complete cure followed its extraction.

2. The Duke of G. Case following exfoliation from the antrum Highmorianum.

3. The Earl of C. Section of the nerves had been repeatedly performed by Sir Everard Home and Mr. C. Bell. Apoplexy supervened, and the sufferings became less severe.

In this case there was an exfoliation of the alveolar processes, to which Sir H. attributed the torments of the patient. The relief he supposed due either to the cessation of this exfoliation, or to the apoplectic shock; but he remarks that there may have been disease of the bones of the head.

4. Dr. P. Died of apoplexy. The os frontis was found of unusual thickness, and there were osseous growths on the dura mater; he had had suppuration in the frontal sinus. Sir Astley Cooper had divided several branches of the fifth pair. Sir Astley showed Sir H. Halford the skull of another person who had died of *tic douloureux*; the internal surface of the frontal bone was "a perfect rock-work."¹

Dr. Bright has seen at least one case confirming the opinion of Sir H. Halford.² Mr. Brodie has also seen two or three similar cases, but considers them of rare occurrence.³

Under the same head we may consider the neuralgic pains produced in certain instances by the passage of calculi. A very interesting case of this kind is recorded in the *Bulletin Général de Thérapeutique*,

¹ Lond. Med. Gaz. Vol. I.

² Ibid. Vol. X, p. 327.

³ Lect. in Lond. Med. Gaz. Vol. XVII.

for May, 1836. The patient was a gentleman, fifty-two years of age, of robust constitution, who had previously suffered in two instances from gonorrhæa, and who had experienced frontal neuralgia, which readily yielded to cinchona. On the 4th of March, 1835, he was suddenly seized, being at the theatre, with pain in the left testicle, propagating itself to the spermatic cord, with general spasms, repeated vomiting and extreme anguish. The symptoms ceased after fifteen hours, but reappeared on the third day, and continued under the tertian type. Under the use of quinine they were suspended for a month, and then returned with irregular intervals. Almost every medicinal agent was tried in vain, and the patient at last left Paris, trusting to time, "the happiest of physicians." At last a small calculus was passed through the urethra, and the pain ceased immediately and entirely.

It has been often asserted that neuralgia of the face has been caused by the presence of carious teeth. Mr. Bew, in his treatise on *tic douloureux*, declares, that a great proportion of the cases which he has seen, arose from the irritation produced by the teeth.¹ Mr. Bew, I believe, is a dentist. But Mr. Brodie declares positively that he never knew a case, where genuine *tic douloureux* was relieved by the extraction of a carious tooth, and a very experienced dentist informed him that he had arrived at the same result.²

In two cases given by M. Piorry, relief of pains having some of the characters of *tic douloureux* fol-

¹ Foster, Dissert. in Am. Med. Review, for Aug. 1826.

² Lect. cit.

lowed the extraction of one or more of the molares.¹ Instances of its failure might be accumulated to any extent ; I will only refer to the case quoted by Good,² from the *Zoonomia*, in which two teeth were removed and the antrum was perforated without success, and to another of suborbital neuralgia, cited by Halliday from Wepfer, in which the husband of the patient pulled out successively all the teeth of the upper jaw to the last molaris, besides removing a portion of the gum opposite to the canine tooth and small molares, without any permanent advantage.³

A case is given by Langenbeck, in which tic douloureux evidently had its origin in the operation of removing a tooth. "A man, thirty-three years of age, who had always enjoyed good health, was treated at our academic hospital for a painful affection of the face. He had suffered from this dreadful malady during five years, having been attacked with it after the extraction of one of the molar teeth. The pain began in the socket from which the tooth had been removed. It then attacked the nostrils and the eyes. After four years it passed from these parts to the cheek and the ears. The patient suffered at intervals and for a short time only ; the pain was at first pungent, and then burning, as if from fire ; the lancinations occupying the region of the nose, of the eyes, of the cheek, of the ear, and thence passing to the lower jaw, kept up a perpetual motion in the muscles of the face ; the masseter being rendered very hard

¹ Obs. 6th & 7th.

² Vol. IV, p. 218.

³ See also the case of Mrs. W., in Dr. Jackson's paper on tic douloureux. (*N. E. Med. and Surg. Journal*. Vol. II, p. 121).

and contracted, did not allow the mouth to be fully opened. On opening it, or passing the finger into the mouth, the pain instantly commenced. The infraorbital nerve was therefore divided, which operation will be described in our remarks upon the section of the nerves."¹ A case is mentioned by Dr. Jackson, in the paper already referred to, in which the painful affection accompanied the protrusion of a tooth, and ceased under the influence of conium. (Case of Mrs. S.)

How far some of the different mechanical causes mentioned act by first inducing inflammation in the nerve affected, I will not pretend to decide. Mr. Ley doubts, if tension or pressure of a *healthy nerve* is sufficient to produce pain, but as this is not a matter of very great practical importance, I refer to the papers already cited for the development of his opinion and arguments.

Another cause which has been assigned for neuralgia, is the inflammation of a mucous membrane in the neighborhood of the parts affected; as of the pituitary membrane in the case of *tic douloureux*.² I mention this among the local causes, because the branches of the nerves affected are distributed to the diseased part. In Dr. Powell's account of the influenza of 1807, as it appeared in Clinton County, N. Y., (Med. Rep. Vol. VI, Hex. 2, p. 347,) certain symptoms are mentioned which have a neuralgic aspect. "Sept. 11th the influenza still raged. Many were at this time seized with a pain in one or both ears; some

¹ Tract. Anatom. Chirurg. de Nervis Cerebri in dolore faciei consideratis. Gottingæ, 1805, p. 39.

² Dict. de Méd. et Chir. prat. Art. Névralgie.

with pain in one or more teeth, especially if any were carious ; and some were first affected with a severe pain in the small of their back ; others in the back part of their head, complaining that the tendons of their neck were sore," &c.

During the prevalence of the influenza within the past year, a case occurred to me which I will briefly mention. Mr. D. applied to me for advice, as he was suffering from the common symptoms of the complaint, and in addition from some soreness of the mucous membrane of the mouth, which was a little reddened, and I believe excoriated over the palate. I ordered the remedies used in ordinary cases. On meeting him about a week afterwards, he told me that he had been suffering greatly in the interval from a throbbing pain in the anterior part of the lower jaw, of such intensity that he could hardly help crying out with the suffering. This pain came on regularly for four or five nights at about twelve o'clock, and lasted until three. He anticipated its return with so much certainty, that he sat up instead of going to bed. He applied to a dentist, who attempted to extract the second molaris of the left side, which broke under the operation. Creosote was applied to the broken tooth, and the patient took some purgative pills, but that night the pain was as bad as ever. The pills operated the next morning, and the following night was passed very comfortably. Since this, he had been free from suffering. Almost at the same period I saw two other cases, in which the pain was very intense, and presented many of the characters of *tic douloureux*, in the one case radiating over a considerable part of the face, but not enduring many days ; in the other being very obsti-

nate, and for some time defying both the physician and the dentist. I mention these instances rather as a hint to other observers, than as attaching much value to facts so wanting in detail.

Having considered the principal causes supposed to produce neuralgia, by acting upon the nerve affected, or its ramifications, I arrive at the opinions of those who have placed the seat of the disease in the nervous centres; the spinal marrow, the ganglions, or the brain. Each of these organs has been considered as the seat of those changes which produce neuralgia in the nerves proceeding from it.

The opinion that the origin of this affection is in the spinal marrow, was long ago brought forward by Lentin; an hypothesis which is mentioned and combated by Langenbeck.¹

The little work of Mr. Teale, on the diseases which he has called neuralgia, with what propriety we have already examined, is well known to the medical public. None of the nineteen cases given by this author, are accompanied with post mortem examinations, as all of them terminated favorably. It is from certain symptoms, and results of treatment, then, that he has been led to consider the cause of the different affections he has noticed, as seated in the spinal marrow or ganglia, the morbid condition of which he calls irritation. The most remarkable of these symptoms, is thus mentioned. "In this complaint, tenderness in the portion of the vertebral column which corresponds to the origin of the affected nerves, is generally in a striking and unequivocal manner evinced by pressure. In some in-

¹ Op. cit. p. 33.

stances the tenderness is so great, that even slight pressure can scarcely be borne, and will often cause pain to strike from the spine to the seat of spasm or neuralgia."¹

"In conjunction with the symptoms denoting disease of the ganglia, tenderness to a greater or less degree may generally be found on pressing some part of the spine, and the tender portion invariably corresponds with the symptoms; or rather, the seat of tenderness is near the part occupied by the particular ganglia from which the nerves of the disordered organ are derived; for example, when the heart is affected, the tenderness is found in some of the cervical vertebræ, and when the stomach is the seat of complaint, it is in some of the middle or lower dorsal vertebræ."²

"When the different neuralgic symptoms which have been enumerated can be traced to this morbid state of some portion of the spinal marrow, the treatment that ought to be pursued is readily decided upon. Local depletion by leeches or cupping, and counter-irritation by blisters to the affected portion of the spine, are the principal remedies. A great number of cases will frequently yield to the single application of any of these means."³ And in the case of ganglionic irritation, "leeching, cupping, blisters, &c. to the neighborhood of the affected ganglia, constitute the essential part" (of the treatment). Opinions of like nature have been entertained by the Messrs. Griffin, by Dr. Brown, and by Dr. Darwall.⁴ Mr. Tate professes to have discovered a similar affection, characterized by

¹ Treatise on Neuralgic Diseases, p. 14.

² Ibid. p. 41.

³ Ibid. p. 19.

⁴ Lond. Cyc. Art. Diseases of Spin. Marrow.

tenderness on applying heat or pressure over the vertebræ, in hysteria, but he does not pretend to offer any hypothesis respecting the morbid condition of the spinal marrow. He remarks of the treatment, "upon the discovery of this tenderness between the scapula, I have frequently leeches the part, which has always failed to afford any important relief: I have afterwards blistered the spine, without deriving therefrom, at best, more than partial benefit; but I cannot too often repeat, that the antimonial eruption exerted always a most powerful influence over the disorder."¹

The existence of such a condition of the spinal marrow or ganglia in neuralgia being an inference, and not a demonstrated pathological fact, I shall give it a brief examination.

Mr. Teale himself, could hardly avoid seeing an obvious difficulty in the way of the reception of this doctrine, which difficulty consists in supposing that pressure, and even very slight pressure can be transmitted to the spinal marrow through the vertebræ. To this he replies, that "the affection most probably is not confined to the spinal marrow and its membranes, but that the surrounding tissues, as the ligaments, the cellular connecting medium, &c., are all involved in a state of phlogosis."² According to one of the zealous advocates of spinal irritation, "the doctrine is not that the spinal chord is compressed, but that the portion which is diseased, conveys to the nerves proceeding from it, its own sensitiveness, and the covering of the vertebræ being supplied therefrom, these nerves, the shortest conductors of im-

¹ Treatise on Hysteria, p. 48.

² Op. cit. p. 67.

pressions to their centres, by their own condition indicate that of their origin.”¹ Another irritationist writes as follows: “The intervertebral foramina are of considerable size, and are filled up by a loose cellular tissue surrounding the nerves and bloodvessels; so that perhaps it is not impossible that the chord itself may receive some portion of the pressure. If, however, it is so well protected that no part of the force can be applied directly to it, through the intervening parts, we must suppose that pressure upon the roots of the nerves after they have passed out from the canal, is sufficient to produce the pain and contractions, to which we have referred; and this answers every purpose in establishing our diagnosis, inasmuch as we have no proof that the nerves immediately exterior to the canal are ever affected, independent of the chord.”²

Let us first examine the facts upon which this doctrine of spinal irritation must rely. We cannot deny that certain nervous and hysterical complaints have been accompanied with tenderness in the region of the spine. The ardent writers to whom we have referred, have reported many cases of this kind, which in this point are worthy of full confidence. But is this tenderness constant in such affections? Dr. Abercrombie, whose authority is unexceptionable, in speaking of this very class of cases, declares that, “there is no doubt that these affections often appear, without any symptom that can be referred to the spine, and in

¹ Nichols on Irritation of the Nerves. Med. Com. of Mass. Med. Soc. 2d Series, Vol. I, Part VI.

² Obs. on Spin. Irrit. and Inflamm. by Sam. Annan, M. D., of Baltimore. Am. Journ. of Med. Sciences, May, 1837.

many cases with sources of irritation distinctly referable to other organs.”¹

The statement which follows, is from a review of one of Dr. Griffin's memoirs by M. Pidoux.² The reviewer remarks, that two or three years previously he had met with the idea in some English author, that rheumatism of the joints proceeded from an affection of those points of the spinal marrow, from which the nerves of the diseased articulations were supplied; which condition of the spinal marrow was inferred from tenderness at the corresponding parts of the vertebral column. He asserts that he was only once able to find a case supporting this notion, although he and others examined the spine in many cases of rheumatism. He therefore suspected the value of the new assertion, but as analogy might be deceptive, he had recourse again to experiment. “I therefore,” he says, “explored by pressure and even by percussion, the spines of more than twenty women, affected either with different neuralgiæ, especially of the face, or with gastralgia, or with palpitations, or finally with one or several of the nervous diseases which Mr. Griffin asserts to have their origin in an irritation of some portion of the spinal marrow, and I have only been able to obtain negative results, *I repeat it, absolutely negative.*”

In the second place, it is not always stated how far other regions of the body were the seat of similar tenderness. Yet it is sufficiently apparent, from the examination of some of the cases, that the spinal region is by no means the only one which becomes extremely

¹ On Diseases of the Brain and the Spinal Cord, p. 339.

² Journal des Connaissances Méd. Chir. Sept. 1835.

sensible to pressure. Thus it is sometimes the breast,¹ at others the intercostal spaces,² or the knee,³ or a spot beneath the margin of the ribs,⁴ or the supra-orbital, the infra-orbital, the inferior maxillary nerves, or "almost every nerve in the body at the same time,"⁵ or there is even pain shooting to the neck and head, and down to the breast, upon touching the tips of the fingers.⁶ In such cases there is certainly strong reason to suppose the nervous extremities themselves the seat of the morbid sensibility, since their distance from the spine prevents the supposition of any direct influence upon the latter, or its envelopes, or even the origin of the nerves, and therefore the effects attributed to pressure over the vertebræ could not be anticipated.

In the third place it is not clear that tenderness of the parts covering the spine does not frequently exist in persons who are unaffected with any of the symptoms supposed to attend it. Tenderness of the epigastrium has been sometimes considered a very conclusive symptom, yet, if I remember correctly, a celebrated physician has remarked that, if a rank of soldiers are called out and submitted to examination, few of them will bear pressure in this region without shrinking.

Again the facts respecting the treatment require re-examination. Dr. Abercrombie, who is somewhat sceptical upon this whole doctrine, relies much less confidently than the firm believers, upon the local applications to the spine. In one form of what Mr. Teale and Mr. Tate would call the spinal affec-

¹ Teale, Case 5th and 6th.

² Case 7th.

³ Case 9th.

⁴ Tate, Case 4th.

⁵ Ibid. p. 92.

⁶ Teale, Case 6th.

tion, he recommends purging, tonics, and anti-spasmodics, strong friction, cold spunging or shower bath, and last of all blistering upon the spine. Of another form he says, "I have treated such cases by topical bleeding and blistering without benefit; and I have generally found the most effectual treatment to be regular but moderate purging and sea-bathing."¹ Dr. Todd allows that, "instances are by no means wanting where these remedies (leeching and counter-irritation) have totally failed in removing the symptoms in question."² But the most singular circumstance in the evidence, is the discrepancy between Mr. Teale and Mr. Tate. Both of these authors attach the highest importance to tenderness over the vertebræ, as the sign of an affection of the spinal marrow. According to the first, "local depletion by leeches or cupping, and counter-irritation by blisters to the affected portion of the spine, are the principal remedies."³ But what says Mr. Tate? "I have frequently leeches the part, which has always failed to afford any important relief; I have afterwards blistered the spine, without deriving therefrom, at best, more than partial benefit."—Again, "it is singular that it (the antimonial pustular eruption) should be followed by such decided effect, after leeches, blisters, cupping, and the like, had failed to produce any influence over these disorders."⁴ Yet these two observers thus flatly opposing each other in their results, were evidently occupied with the same class of cases, as may be

¹ *Op. cit.* pp. 333, 338.

² *Lond. Cyc. Art. Diseases of Spinal Marrow.*

³ *On Neuralgic Diseases*, p. 20.

⁴ *On Hysteria*, p. 48.

easily ascertained from an examination of the cases themselves; indeed Mr. Teale considers Hysteria as a fashionable misnomer for an assemblage of neuralgic symptoms.¹

But allowing that in a certain number of cases, there is tenderness over the vertebræ corresponding to the origin of the nerves; and conceding that some or many of these cases have yielded to different remedies applied over the affected region, this is not sufficient to prove any affection of the spinal marrow, and still less an inflammatory, or irritated condition. If those who are little familiar with anatomical investigation, should still suppose that the spine may be directly acted upon by pressure of the vertebræ, a few considerations will show the unreasonableness of this idea. In the words of the French reviewer of Dr. Griffin, "it would seem less improbable, did we accuse the violent movements of the neck and of the trunk of increasing the pains, although nature has used a multitude of ingenious precautions to guard the spinal column and the nerves issuing from it from compression, from tension, and from the torsion they might have suffered in the extensive movements of the spinal column."

In addition to the size of the vertebræ, the solidity of the intervertebral substance, the rigidity and strength of the numerous ligamentary bands of union, the masses of muscle by which the spine is, as it were, cushioned and protected, let us remember the following anatomical arrangement, which I quote, not in the words of a disputant, but from a general treatise on physiology.

¹ Ibid, pp. 36, 72.

"The canal formed by the dura mater around the spinal marrow, which is lined by a double layer of the arachnoid, is much larger than is necessary to contain the organ; so that in the dead subject there is an empty space between the spinal marrow and its membranous envelopes. I call this space the subarachnoid cavity; but during life this cavity is filled by a serous fluid which distends the membrane, and which darts frequently to the height of several inches when a small puncture is made in the dura mater."

—"It is easy to see what efficacious protection the spinal marrow receives from the liquid which surrounds it, and in the midst of which it is suspended like the foetus in the uterus, with this difference, that it is fixed in its central position by the ligamentum dentatum and the different spinal nerves."¹

As to Mr. Teale's explanation, that "the surrounding tissues, the ligaments, the cellular connecting medium, &c., are all involved in a state of phlogosis," it resolves itself into two propositions; first, that the spinal marrow itself is inflamed, and secondly, that this condition is propagated to the neighboring parts through the ligamentum dentatum, or the nerves and their accompanying vessels and envelopes. The first is a pure assumption, and in order to show the reasonableness of the second, it must be proved that this inflammation, confessedly of the lowest degree, is capable of extending itself through the slender media of connection between the spine and the surrounding organs. But in addition to this, the skin is in some of these cases so tender, that the patient shrinks from

¹ Magendie, *Précis Élément. de Physiologie*, Paris, 1833, Vol. I. p. 224-25.

the slightest contact, nay, even from brushing the light hairs on the nape of the neck ;¹ yet there is no pretence that the skin is either reddened, or hot, or tumefied, and therefore we cannot pretend that it is in any degree inflamed.

The other supposition is, that the branches or roots of the spinal nerves are the seat of the increased sensibility, which is felt, either in the parts to which the nerves are distributed, or near their origin, where they pass through the intervertebral foramina. But does this sensibility prove that the spinal marrow is affected ; or may not the nerves themselves be affected, independently of the spinal marrow ? Dr. Annan's proposition that the tenderness of the nerves, after they have passed out from the canal, "answers every purpose in establishing our diagnosis, inasmuch as we have no proof that the nerves immediately exterior to the canal are ever affected, independent of the chord," would be more logical, if for "*every*" we should substitute *little* purpose, and for "*are ever affected,*" *are not frequently affected*. The plain statement of facts is this ; different parts of the body, in a certain class of patients, become the seat of neuralgia and spasm ; certain parts, as the breast, the thoracic muscles, the integuments and muscles of the back, are more or less frequently the seat of increased sensibility to pressure and heat ; when the anterior branches of a given spinal nerve are peculiarly affected, it is not uncommon to find the posterior branches, which are distributed to the soft parts about the spine, morbidly sensitive. But merely because many branches springing from a common root are affected at the same time,

¹ Dr. Annan's Obs. on Spin. Irrit. and Inflamm.

it does not follow that their common source is diseased; the only accurate deduction that can be drawn is this;—two branches which spring from a common root are more liable to be simultaneously affected than two which spring from different roots. But the susceptibility of disease belonging to certain portions of the nervous system, whether the fifth nerve with its three branches, or one of the spinal nerves with its two great divisions, the anterior and posterior, does not in the least prove that the part in which they take their origin is the seat of any disease whatever. The simultaneous manifestation of disease in the anterior and posterior branches of a spinal nerve, is very possibly owing to an impression which they receive in common, acting upon a susceptibility belonging to all the branches which unite in a given portion of the spinal cord.

But the term “spinal irritation,” which has been applied to cases presenting these phenomena, conveys the assumption, that the spinal marrow is the origin of the symptoms, and even gives a name to the supposed disease before it is shown to exist; it may well be questioned, therefore, whether it should at present be employed in science.

With regard to the proof furnished by the effects of treatment, I will bring forward but a few objections. First, the facts prove a great deal too much. The effects of leeching and blistering (which act a little more directly on the spinal marrow than a cataplasm on the mother’s abdomen does on the fœtus in utero)—are altogether too rapid and successful in their results, to suppose that there was really *irritation*, that is, according to Mr. Teale, a shade of inflammation, in the

delicate and deeply seated organ suspected. Mr. Teale declares, that a great number of these cases will frequently yield to the single application of any of these means. Some cases, he says, which have even existed several months, I have seen perfectly relieved by the single application of a blister to the spine, although the local pains have been ineffectually treated by a variety of remedies for a great length of time.¹ But is it true that other chronic irritations and inflammations are in the habit of yielding at once to a single application of leeches, or a single blister? On the contrary take the mildest form of chronic sub-inflammation of the conjunctiva, an affection in which we can apply our remedies very near the diseased part, or the mildest chronic inflammation of the skin, in which the leeches may drink directly from the turgid vessels, and our blisters may encircle or cover, if we please, the affected part—is it usual in these cases to produce such magic effects by a single prescription? Such has not been our experience; how singular then that so perfect a control should be exercised by these means over an organ protected by skin, muscles, tendons, bone, ligaments, fibrous and serous membranes, and in the midst of these sevenfold bulwarks, floating suspended in the centre of a fluid; an organ too, which, as if destined to be independent of the surrounding parts, receives its arteries, not merely from a few filaments of the intercostal and lumbar vessels, but two of whose principal trunks (the posterior and anterior spinal arteries) are transmitted from within the cavity of the cranium, and whose

¹ *Op. cit.* p. 20.

venous branches pour their blood, not through the region where the remedies are applied, but into the *inferior veins of the cerebellum*.

But independently of spinal irritation, we can easily conceive that leeches or blistering may have been useful. Suppose there is no disease of the spine, is there no such thing in therapeutics as depletion, or as revulsion? I am perfectly willing to take the facts of spinal tenderness, and the efficacy of leeches and blisters over the vertebræ, for whatever they may be worth, but I freely own that this new creation of medicine, spinal irritation, appears to me a very problematical cause of the numerous symptoms attributed to it. The very loose and unsatisfactory manner in which many of these cases are reported, which often renders it impossible to say how far the recumbent posture, diet, and internal medicines contributed to the cure which is always ascribed to the local remedies, might inspire us with some doubt; a doubt which is confirmed on examining the notion itself in the light of anatomy and physiology.

That the ganglia, situated in front of the vertebral column, can feel the pressure exercised on the vertebræ, appears to me so monstrous a proposition that I am unwilling to examine the extravagance of the idea. All that is practical in the results of the advocates of the doctrine of spinal and ganglionic irritation we may safely receive; we may leech and blister, if we please, from the pons Varolii to the cauda equina; but when we are forming pathological conclusions, let us pay some respect to the common laws of mechanics, and the analogies of diseased action in other parts of the system.

A certain number of cases of neuralgia have been supposed to have their cause, or one of their causes, in a morbid condition of the brain. Such, according to M. Jolly, are the pains referred by individuals to parts which have been removed by amputation, the return of neuralgic paroxysms, by the force of habit, after the section, cauterization or destruction of a nerve, the pains felt on witnessing the sufferings of another, those to which hypochondriacs are subject, those which are felt in the course of the nerves during dreams, and the cases of neuralgia from moral causes.¹ Of the pains referred to an amputated part, it may be said, that they probably arise from an affection of the remaining nervous trunks and branches, and that the patient requires a new education to learn to localise properly the sensation of suffering ; an education parallel to that which all the senses undergo in infancy, and which is found necessary to teach the blind who have been restored to sight, the distances and relations of objects, so that the error of perception no more implies any derangement of the brain, than the errors of the patient recently operated upon for cataract. And with regard to the second instance brought forward, as on the one hand habit alone does not seem to be enough to keep up the affection, since the section and cauterization of the affected nerve has often cured the disease, and as on the other hand it is impossible to say that all the affected filaments have been reached by the knife or the cautery, and even if they have been, a second branch may have become the seat of an affection like the first ; from

¹ Dict. de Méd. et Chir. prat. Art. Névralgie.

these considerations, it is very doubtful if the source of the evil can be attributed to the brain. If, as is asserted, hypochondriacs are peculiarly subject to neuralgic pains, it remains to be proved whether the brain be really the source of hypochondria. Nor is it easy to believe that moral emotions can be any thing more than the exciting cause of neuralgia, which acts only upon those parts of the nervous system already in a condition favorable to the development of the affection. The opinion referred to has been more broadly stated by Broussais and by Mr. Swan.

“In the fixed and active neuroses of the organs of relation” (among which he includes neuralgia) “the capillary circulation is excited, there is congestion, and inflammation and sub-inflammation exist or threaten to be formed in the tissues where the neurosis is manifested, as well as in that part of the cerebral organs corresponding to the nerves of these same tissues; while the intermediate nervous cords only transmit the sympathetic influences from one point to the other.”¹

“Those local complaints which appear to originate spontaneously, or in some cases when a slight wound has been inflicted, I believe to be only symptomatic of a general irritability of the brain and nervous system. The almost constant failure of topical remedies, and of the division of the affected nerve, must lead to the conclusion that the cause of the local diseased action, or primary affection, must reside in some other part of the body; and if we inquire into the causes of the local active affections of the nerves, it will be

¹ Broussais, *Exam. des Doct. Méd.* 3d ed. p. xlvii.

found that the atonic state of the body, or whatever tends to render the brain and nervous system irritable, will generally be found the most frequent.”¹

The opinion of Broussais is an assertion in keeping with his whole system, and it is hard to say upon what proof it is founded. The perception of pain by the brain does not certainly imply any thing like congestion or inflammation in this organ; otherwise, how is it that patients suffer for years from the agonies of neuralgia or of gout, without any sign of cerebral disturbance? Is the inflammatory process transmitted to the brain from the affected nerve? But there is no evidence in the majority of cases that the nerve itself is inflamed. If we look at the effects of treatment, do we not find a hundred cases cured by the carbonate of iron, for one that yields to starvation and leeches? I cannot but consider this dogma of M. Broussais as both theoretically and practically on a par with many of his wholesale axioms, and what may be the effect, in practice, of these *physiological* notions, I leave to be inferred from the following statement of a recent French writer.

“Professor Boyer, whose authority no one will impeach, has informed me, and permitted me to make it public, that he had cured by means of nourishing diet and tonic medicines, *more than thirty* persons who had been reduced to the most deplorable condition by leeches, gum water, milk diet, &c.; and the late Dr. Georget, whose merit and good faith were well known, met with three instances of hypochondriacal gastralgia in which the antiphlogistic

¹ Swan's Diss. p. 37.

treatment resulted in the most complete physical extenuation, coupled with a state resembling imbecility." And in a note he cites the authority of Dupuytren to a similar effect.¹

Mr. Swan's supposition, that neuralgia is symptomatic of a general irritability of the brain and nervous system, is open to several objections. It is obvious in the first place, that this hypothesis does not at all explain why a single nerve should be the constant seat of suffering. It is necessary then to suppose another cause, which localises the painful affection; and this cause, whatever it is, may be sufficient without the presence of the supposed general irritable condition. But another powerful argument may be found by glancing at the history of the disease. If "the atonic state of the body, or whatever tends to render the brain and nervous system irritable," be among the most frequent causes of neuralgia, as Mr. Swan supposes, two affections above all others should be found to produce it; chlorosis, in which atony is carried to so great an extent, and hysteria, in which the brain and nervous system are irritable, if they ever are. We should then expect to find neuralgia more frequently in women than in men, and very common between the ages of fourteen and twenty. But according to M. Jolly, "it is the result of observation that neuralgia affects men more frequently than women; a fact placed beyond doubt by Thouret, who found that out of a given number of

¹ Barras, *Traité sur les Gastralgies et les Entéralgies*. Paris, 1829, p. 49.

cases of neuralgia, more than two thirds happened in men, and one third at the utmost in women.”¹

And with regard to the age most subject to facial neuralgia, we may, with Halliday, adopt the calculations of Masius, which are based upon two hundred cases. According to these, “we find that the very great majority happens between the thirtieth and the sixtieth year.” Out of this number only two cases were found below the age of twenty, and four from twenty to twenty-seven. Besides these facts, it is stated that Fothergill had not seen the disease in a single instance under the age of forty, out of eighteen cases. Bellingeri found only two cases in which it occurred under forty years of age, out of forty patients; and in the same series the male sex was most frequently affected.²

Dr. Armstrong considered tic douloureux, as well as periodical headache, as most frequently arising from congestion or disturbed action in the vessels of the brain.³ He therefore had recourse to decided bleeding and purging, which he assures us were successful in five recent cases. He doubted whether it would be successful in the more chronic cases, which he supposes may be accompanied with structural derangement, the consequence of long continued disordered action. I doubt not that Dr. Armstrong, in the desire to settle the question, took the secondary disturbances produced by the painful affection for the cause of the disorder. However, he says that in all his cases, the affection of the face was *preceded* as

¹ Dict. de Méd. et Chir. prat. Art. cit.

² Annali Universali di Medicina.

³ Pract. Illust. of Typhus and other Fevers, p. 247.

well as attended by clear manifestations of cerebral disease. But we have good evidence that this is not commonly the case, for neuralgia is frequently considered as common toothache at its commencement, so distinctly local is its origin, and is not unfrequently brought on, as well as remedied, by distinctly local agents.

We have besides the same therapeutic results to which we have before alluded, against this notion that the disease consists in inflammation or increased vascular action. But perhaps the best argument against this doctrine is, that in those cases determined either by post-mortem examination or by manifest symptoms, to be actually congestions of the brain, neuralgia is not one of the ordinary symptoms. Paralysis and convulsive motions do indeed take place, but I am not aware that *tic douloureux* has ever been noticed.¹

But independently of causes to be found in the brain and nervous system, neuralgia has been traced to other sources, less immediate in their connection. Dr. Fothergill advanced the supposition that it was owing to a "sharp corrosive *cancerous* acrimony," and supported this idea by a limited number of cases, in which cancerous disease co-existed with the neuralgic affection.² This was deemed by Dr. Haighton,³ a hypothesis of sufficient importance to require a refutation, but can hardly demand more than a passing notice at the present day.

It has been sometimes supposed connected with

¹ Vide Andral, Vol. V. (*Congest. Cerebrales*).

² Med. Obs. & Inquiries, Vol. V, Lond. 1776.

³ Med. Records and Researches, Lond. 1813, pp. 39, et seq.

sypilis. A remarkable case, in which mercury was prescribed successfully, on this supposition, is given by Waton,¹ and has been quoted by Halliday,² and referred to by Langenbeck,³ who argues against the conclusion which might be drawn from this single case. Two similar instances of cures effected by mercurial salivation in France, are referred to by Dr. Jackson,⁴ but there is little evidence that this is a frequent cause of neuralgic affections.

The gouty and rheumatic diathesis have been not unfrequently accused of causing neuralgia. The first of these causes was alleged by Carl Sprengel,⁵ and is enumerated, among others, by Forstmann, Monfalcon,⁶ and Chomel.⁷ Dr. Hosack suggests that neuralgia may be due to the state of the blood-vessels, an *arthritic* habit, or that condition which induces chronic rheumatism.⁸ Barthez had observed sciatica combined with gout and rheumatism.

"Sunt qui pro malo rheumatico habent," are the laconic words of Langenbeck. The affection is supposed sometimes due to this cause by many other authors, among whom are Monfalcon,⁹ Chomel,¹⁰ Bright,¹¹ Piorry,¹² and Bellingeri.¹³ The opinion is apparently most correct concerning sciatica, which particular form of the affection Chomel considers due in a great number of cases to a rheumatic cause, but it is extended

¹ Recueil périod. de la Soc. de Méd. Tom IV, No. XXI, and Journ. de Méd. et Chir. Mars, 1793.

² Obs. 23.

³ Op. cit. p. 37.

⁴ Discourse, &c.

⁵ Langenbeck, p. 37.

⁶ Dict. des Sc. Med.

⁷ Lectures.

⁸ Med. Essays, Vol. II, p. 249.

⁹ Art. cit.

¹⁰ Lectures.

¹¹ Reports of Med. Cases.

¹² Op cit. Case 10th, p. 272. ¹³ Annali Universali di Medicina.

by the Italian author to facial neuralgia. It acquires confirmation from the statement of Monfalcon, that those who are exposed to atmospheric vicissitudes, as sailors and fishermen, are peculiarly liable to the complaint.

I hardly know on what grounds the idea of Langenbeck was founded, that facial neuralgia was allied to, or identical with hysteria. He has brought forward this notion in a half-hesitating manner, which almost excites a smile in the reader of a scientific treatise. "*Ut quid sentio, dicam: dolor faciei pro hysteria partiali haberi posse videtur.*"¹ The fact, that the disease is more common in men than in women, and particularly, that it so frequently attacks fishermen and sailors, who are any thing but hysteric subjects, does not favor this hypothesis. Hysteria is admitted as an occasional cause, however, by Mr. Brodie.²

The suppression of different discharges has been enumerated among the causes of the affection of which we are treating. Some cases of this kind are considered as satisfactorily established by Monfalcon. Sauvages admitted, in his Nosology, an *Ischias verminosa*, or a neuralgia from worms. Like other exceptional or problematical sources of disease, these should be kept in mind, as being possibly of important therapeutic application in certain cases.

Certain pains referred to the cutaneous surface, without obvious organic cause, appear to deserve the title of neuralgia. Such are those which follow after the disappearance of Herpes Zoster.³ Dr. Bright has

¹ Op. cit. p. 39.

² Lect. in Lond. Med. Gaz.

³ Cazenave et Schedel Abrégé prat. des Mal. de la Peau. Paris, 1833. p. 113.

found pains of this kind extremely troublesome and obstinate, and has used various means with little benefit.¹ A case is reported in the *Gazette Médicale de Paris*² from another journal, in which neuralgia of the breast ceased upon the appearance of scabies, and reappeared on curing the cutaneous affection; the patient was only relieved by again contracting the latter complaint. In the seventh case of M. Piorry, a slight eruption of *melitagra flavescens* (impetigo) occurred two or three days after the relief from the neuralgic symptoms, but this may not have been connected with the original complaint.

Neuralgia has been considered as arising from disorder of the digestive organs. This is not a very new idea. “*Volger malum hoc a vitio gastrico effici posse credit.*”³ Mr. Abernethy says, in his *Treatise on the Constitutional Origin and Treatment of Local Diseases*, “In the cases of *tic douloureux* which have fallen under my observation, these parts (the digestive organs) have been greatly deranged; and I have cured patients of such complaints, by correcting this disturbance.”⁴ But, as Mr. Lawrence remarks, in one of his lectures, if they were in a good state at first, the pain will produce disorder of these organs. In a lecture reported in the 17th Volume of the *London Medical Gazette*, Sir Charles Bell advocates the opinion, that one of the forms of neuralgia, *tic douloureux*, is due to derangement of the digestive viscera, and

¹ Lect. in Lond. Med. Gaz.

² For May, 1836.

³ Langenbeck Op. cit. p. 38, (1805). Hamel refers it to the same cause in some cases, (*connexion sympathique de l'estomac avec les autres parties.*)—(*état saburral des premières voies.*)

⁴ Cases, Section 1st.

supports this opinion by pointing out the connection of the fifth pair of nerves with the branches of the sympathetic. In the same volume, Mr. Kerrison asserts a prior claim to this doctrine, which, as he alleges, he brought forward in an inaugural thesis in 1820. After all, however well anatomy may explain the fact, should it be proved, it can do nothing of itself to prove it. But Sir Charles Bell himself allows, that the blue pill and bitter draught will not cure tic douloureux, and Mr. Lawrence remarks sensibly enough, referring to the assertion that this complaint was to be remedied by regulating the digestive organs, that there is no simple mode of treatment, for if there were it would be known, and could be depended upon. How convincing Mr. Kerrison's arguments were, I cannot say, but Sir Charles Bell's principal proof results from his successful treatment of five cases at the utmost, by means of croton oil. I may remark, that the anatomical argument will not apply with the same force to tic douloureux affecting the *facial nerve*; if such a form of the disease exist, as would appear from some of Halliday's cases.¹ Dr. Elliotson declares that, "allowing some of the blame to be deserved, that is laid upon the digestive organs as causing all kinds of diseases of all parts—a vulgar assumption easily made, and saving a world of investigation and accurate reasoning,—we have never seen but one case of neuralgia referable to such an *origin*."²

But however deficient the evidence may be that neuralgia in any of its forms actually results from derangement of the digestive organs, this condition

¹ Obs. 19, 22, 23, and references on p. 96.

² London Cyc. of Pract. Med. Art. Neuralgia.

so evidently exercises a powerful influence over many morbid phenomena, that it gives rise to one of the most frequent practical indications.

The last, but by no means the least important of the conditions giving origin to neuralgia, is the state of the system, or certain parts of it, produced by malaria. The opinion that neuralgia is a form of intermittent fever, or rather that it recognises the same specific cause, has been recently brought forward by M. Jolly in France, and Dr. Macculloch in England. Neither is this a new doctrine. “*Van Hildenbrandt autem pro febre intermittente larvata habet.*”¹

The well known fact that neuralgia often assumes the regularly intermittent form, and yields to antiperiodical remedies, might have led to the supposition that it is frequently owing to the causes which produce other periodical affections. I am not aware, however, that this doctrine has been fully developed before the time of the two recent authors to whom I have referred. The work of Dr. Macculloch on malaria,² and the memoir of M. Jolly on neuralgia and intermittent fevers,³ were both published in the same year, so that I know not which claims the precedence. Dr. Macculloch uses these words in the essay referred to.

“An attention to this subject for a very long course of years, has proved to me, that, from whatever other causes it (*tic douloureux*) may sometimes arise, it is one of the disorders produced by malaria, and that

¹ Langenbeck, *Op. cit.* p. 37.

² *Essay on the production and propagation of Malaria.* London, 1827.

³ *Mém. sur les névralgies et les fièvres intermittentes.* Nouvelle Bibliothèque Médicale for 1827.

moreover it is very often a mode of intermittent fever; a chronic disease of this nature, attended by a peculiar local affection.”¹

In a work which he published in the following year,² this opinion is brought forward much more fully, and supported by a considerable number of facts. One or two of the most remarkable I may relate in the author's words.

“In this case, the situation was so decidedly subject to malaria, that scarcely an individual, out of many different families which had resided in it, had escaped intermittent at some period of their stay. In one season, and in one family consisting of twelve or fourteen persons, the following were the effects in as many individuals. One tertian; one double quotidian headache; one diseased spleen; in one individual, aged only eighteen, a temporary hemiplegia with obscure quotidian and symptoms of diseased spleen: a regular neuralgia of the face, of double tertian type. In a following distant season, and in some of the same persons there occurred; palsy of the face with imperfect speech, an attack lasting beyond a week, and replaced by quotidian neuralgia (tic); a double tertian, common intermittent, terminating in a quotidian, or double tertian, neuralgia; a quotidian with neuralgia in the shin bone; the same patient having had, in a preceding season, a common tertian so obscurely marked, that he was ordered to Italy for a consumption (a consumption which was cured by two ounces of bark and a change of place

¹ P. 202. (American edition).

² Essay on Marsh Fever and Neuralgia. London, 1828.

to ten miles distance), and in a following one, having been attacked again with a double tertian, of which one fit was attended by the neuralgia of the shin, and the other by a headache."¹

Another curious instance is that of an individual with whom the author cited was in habits of intimacy.

"In mere fever, this patient experienced various remittents, together with tertian, double tertian, quotidian, and double quotidian, in different years; and, in the anomalous varieties, what may perhaps be referred to the *Asthmatica*, and to the *Stranguriosa*, and also what may possibly be the *Nephralgica* of Sauvages; together with the *Emetica*, the *Hysterica*, and the *Soporosa* of the same arrangement. These intermittents also, at different times, were united with, or succeeded to, or were replaced by periodical and marked general chronic rheumatism, periodical local rheumatism in a limb, and rheumatism of the face, with repeated slight attacks of the ophthalmia of one eye, attended by hemicrania. In simple neuralgia, this patient also experienced that of the face, repeatedly, long relapses of pure hemicrania, clavus, that of the eye or optic nerve, sciatica, and a similar affection in one radial nerve and in the anterior crural; as, on different occasions, he suffered quotidian intermittent toothache, and the most severe neuralgia of the heart which I have ever witnessed, recurring annually for many years, replacing once, a local periodical rheumatism, and more than once replaced and cured by a quotidian simple intermittent."²

Of the community of local origin of neuralgia and

¹ Op. cit. p. 389.

² Ibid. p. 391.

intermittents he remarks, "If I think that it can easily be verified in the unhealthy districts of England, now that the facts and the theory are pointed out, and if I also think that it will be so confirmed, it is even more probable that this will be effected, with even greater ease and in a greater mass, in the pestiferous districts of France and Italy."¹ He adds that the simplest forms of neuralgia have been overlooked by the physicians of those countries, but that if his opinions should reach so far, he is willing to trust to the evidence they shall afford, for the fate of the question.

The following paragraph I translate from the article of M. Jolly in the *Dict. de Méd. et Chir. Pratiques*.

"A fact which contributes not a little to establish the identity between these affections, is that, according to the tables of comparative medical statistics, the regions where intermittents are most frequently observed, are also those where neuralgia is most frequently met with. (Hildenbrand, Nepple, etc.) Another corroborating circumstance is that, as we have already observed, neuralgia as well as intermittent fever is more rare in children and aged people, than in those of middle age; and finally, we shall see that the results of therapeutics in the two cases, equally support this opinion. In fact there is no one of the remedies considered important in neuralgia of the cerebro-spinal system which has not been applied in the same manner, and which has not produced the same curative effects in intermittent fevers, whether of the simple or malignant kind. I have previously published a pretty large number of facts tending to

¹ *Op. cit.* p. 390.

support these analogies, for which I have not sufficient space in the present article (Vide Nouv. Bibl. Med. for 1827 and 1828); but time has only strengthened in my mind the truth of this proposition, that *there exists a perfect identity between the neuralgia and intermittent fevers.*"

Bellingeri takes a similar view of neuralgia; he considers the disease more frequent in Piedmont than in the French climate,¹ but questions whether this be not owing to the irregular temperature of spring and autumn; at which seasons, particularly the former, the disease most commonly has its origin. In the fourteenth volume of the London Medical Gazette, two cases of disease produced by miasmatic exhalations are reported from an American Journal, one of which may be considered as an instance of neuralgia. I believe enough has been said to support the proposition that malaria is among the causes which produce neuralgia; a fact which leads to important consequences with regard to its prevention and treatment.

¹ Annali Univ. di Med. (Vide Journ. des Connaissances Méd. Chir. for Aug. 1834).

TREATMENT OF NEURALGIA.

If, as I have attempted to show, the nature of this affection is not always identical; that is, if it do not always recognise the same condition of parts or the same external causes, it cannot be expected that any one method of treatment should rank as the best, but our choice must be governed by certain circumstances. The consideration of these circumstances gives rise to a number of distinct *indications*. But independently of the plans of treatment based upon these indications, many remedial agents approved by experience, but whose manner of action is not at all comprehended, must be included in the catalogue of our resources. I shall then pursue the method adopted by Halliday, in dividing the treatment of neuralgia into *rational* and *empirical*. The rational treatment is that which is pursued in accordance with definite indications. I will endeavor to expose these indications in a series of distinct propositions.

1. Indications derived from the ascertained condition of the nerve affected, or its ramifications. Thus if the signs of inflammation be present (continuity of the pain, tenderness on pressure, increased size of the nerve) the antiphlogistic treatment is indicated. If the nerve be pressed upon by a solid tumor, its extirpation, if possible; if by an aneurism, the ligature of the arterial trunk; the removal of foreign substances, of carious teeth, of cicatrices,¹ of stercoral concretions, or of the enlarged ends of the nerves after

¹ See Verpinet's case in Ed. Med. and Surg. Journal for 1807.

amputation ; in case a nerve have been partially divided its complete division, &c.

2. Indications derived from the *supposed* state of the spinal marrow or the brain. Topical remedies ; general depletion, &c.

3. Indications derived from the disturbance of organs sympathetically connected with the nervous system. Regulation of the digestive organs, if deranged.

4. Indications derived from the existence of certain affections considered as constitutional. The employment of anti-rheumatic and anti-arthritic remedies ; of mercury in syphilitic cases ; of bark in the state produced by malaria.

5. The existence of the periodical form, presents by itself a sufficient indication for the employment of cinchona, arsenic, &c.

6. Indications offered by the apparently metastatic origin of the affection. The recalling of suppressed discharges or eruptions.

7. Removal of the patient, if possible, from the influence of all external causes, as of atmospheric vicissitudes, marsh miasmata, sources of moral excitement, &c.

I shall now briefly consider the results obtained by fulfilling the most important of these indications.

According to Martinet, the antiphlogistic treatment almost constantly succeeds in the inflammatory cases of neuralgia ; while, on the other hand, in cases of pure neuralgia, we derive little advantage, if not positive injury from local and general depletion, and derivatives, which are sometimes useful, at other times considerably exasperate the symptoms.¹ In the first

¹ Mem. cit. p. 353.

case which he reports, relief could only be obtained by keeping the arm and forearm immersed in warm water. In his third case, twelve leeches followed by a cataplasm sprinkled with laudanum were applied to the arm upon the eighth day, and followed by relief upon the ninth. The cataplasms were continued, and the patient was soon cured. In the fourth case, relief repeatedly followed the application of blisters, but the case at last proved intractable. Vesication was without effect in the seventh. Leeches were twice employed with relief in the eighth. In the tenth, a case of sciatica, in which the ileo-femoral articulation was disorganized, the moxa was employed without effect. In the eleventh case given by Mr. Swan, the treatment consisted in cupping, followed by a large blister and the internal exhibition of calomel, the compound powder of ipecachuanha and a purging mixture. By a repetition of these means, the complaint, which was considered an inflammatory sciatica, was removed in a few days. In the case of injured sciatic nerve, of which I have previously given the abstract, general blood-letting was repeatedly employed. I will add the general precepts for the treatment of neuritis as given by M. Jolly.¹ "The treatment of neuritis consists principally in local bleeding more or less abundant and frequently repeated, according to the violence and obstinacy of the pain; in emollient baths and cataplasms, and other general and local antiphlogistic remedies. When the acuteness of the irritation is subdued by time, or the energetic use of antiphlogistic means, recourse is had, with

¹ Dict. de Méd. et. Chir. prat. Art. Névrite.

some advantage, to cupping, sudorifics, sulphur baths, preceded by douches directed to the situation of the pain, and then to blisters, cauteries, moxas, &c. If there is a complication of neuralgic and inflammatory elements, as is sometimes observed, antiphlogistics and anti-spasmodics are to be simultaneously or successively employed."

We shall be obliged to recur to the consideration of some of these remedial agents, because they are employed not only in cases acknowledged inflammatory, but also in cases where no inflammatory element is apparent; that is, empirically as well as rationally.

Mr. Swan's fourteenth case is an instance of the cure of neuralgic pains by the removal of a tumor. I have already referred to different cases in which the removal of foreign substances, of carious teeth, of stercoral concretions, were followed by the relief of neuralgic symptoms. In the case of neuralgia following amputations, the means had recourse to, are the removal of a part of the stump, or the excision of a portion of the nerve. But these painful operations are not always successful. In the case of Anna Allen, before referred to,¹ resection of the stump was first performed, then, by Sir A. Cooper's advice, excision of an inch and a half of the sciatic nerve, and finally amputation at the hip joint, which last operation appears to have been successful. And in another case mentioned in the same communication, a second amputation of the forearm was performed by Mr. Langstaff at the elbow joint, and the arm was after-

¹ *Lancet*, Oct. 8, 1836.

wards removed by Mr. Bransby Cooper at the shoulder joint.

For the results obtained by following the indications presented by the supposed state of the spinal cord or the brain, I refer to such evidence as may be found in the authors whom I have mentioned as holding these opinions.

I am unable to cite more than the general statement of Abernethy, and the few facts given by Sir Charles Bell, in support of the efficacy of means directed to the deranged state of the digestive organs. It is so rarely that these means are employed alone, that it is difficult to judge of their value. In a case of lumbar neuralgia, published by M. Coussays, emetics were used on account of the gastric symptoms, in conjunction however with anti-spasmodics and local applications, with success; and in one of sciatica, from M. Pinel, the intervals were longer under the use of purgatives, but whether from their mechanical effect or not does not appear.¹

I have spoken of the opinion that neuralgia was sometimes of a rheumatic nature. M. Chomel, who believes sciatica frequently of this character, states that vapor baths, Dover's powder, and blistering are useful.² According to Monfalcon,³ Barthez treated sciatica *complicated* with rheumatism by blood-letting, if any habitual discharge had been suppressed, and often with other derivative and local evacuants; by purgatives and emollient enemata, resolvents with sedatives, preparations of antimony with mercury, and

¹ Vide Dict. des Sc. Méd. Art. Névralgie.

² Lectures.

³ Dict. des Sc. Méd.

topical applications to remove the engorged condition of the parts. Some examples of success, and the great name of Barthez, he observes, have secured advocates to this method. Dr. Hosack, with a similar view, recommends the lancet, alterative drinks, and the volatile tincture of guiacum. A case of rheumatic origin is reported by Halliday¹ from Masius, in which an almost immediate cure followed the use of camphor in large doses.

Regarding the cure of neuralgia of syphilitic origin, in addition to the case already mentioned from Waton, I may refer to another adduced from the same author by Boyer, with two others from Weisse and Stark, in which mercurials were successfully employed, but in which it does not appear that there had existed any syphilitic affection.²

In those cases depending upon the condition induced by malaria, or in those in which the affection is regularly periodical, the utility of cinchona or the sulphate of quinine, is so well known, that after the example of Halliday, I shall not encumber my pages with evidence on this point. This author concludes, from the cases that he has examined, that it is necessary to continue the remedy long after the apparent cure, suspending it for a day or two at a time, if we please, and that it should be employed in *enormous* doses, which should be arrived at rapidly rather than gradually.³

The preparations of arsenic, according to Halliday, have sometimes produced remarkable cures, but are

¹ Op. cit. p. 152.

² Traité des Mal. Chir. Vol. VI, p. 347-48.

³ Op. cit. p. 138.

frequently obliged to be suspended on account of the accidents they occasion. Several of the authors whom he mentions have found it diminish, but not cure the complaint; one of them found no advantage from it.¹ Boyer considers the arseniate of potass at least useless when the paroxysms are entirely irregular, and much less efficacious than cinchona when they are periodical. He speaks of it also as a dangerous remedy.² A case of frontal neuralgia affecting the quotidian type is related in a French journal,³ which rapidly yielded to the influence of *salicine*, the medical principle of the willow, already ascertained to be useful in intermittent fever.

Several instances of the successful effects produced by recalling suppressed eruptions are recorded. I have referred to a case already, in which neuralgia of the breast alternated with scabies; a very similar one is mentioned by Halliday.⁴ He also mentions a case from André, in which the attacks returned whenever the discharge from an erysipelatous surface was suppressed. A very curious example of a natural cure by means of a vesicular or pustular eruption is cited by Monfalcon, from the *Journal Gén. de Médecine*. Another case is given by Halliday, in which relief followed the reappearance of an eruption supposed to have been brought back by the use of the *dulcamara*. The following fact is from the same source. "A lady had a pustular eruption upon the forehead and right cheek; it disappeared under the use of an ointment. Some days after, the right eye

¹ Ibid. p. 139.

² *Traité des Mal. Chir. loc. cit.*

³ *Journ. des Connaiss. Méd. Chir. Oct. 1834.*

⁴ P. 133.

became watery, and an acute pain was felt in the course of the frontal nerve; the paroxysms were at first rare, but became more frequent. Many remedies were used without success; at last the tartar emetic ointment was employed, and the neuralgia ceased on the appearance of the pustules.”¹

The propriety of removing the patient from the influence of external causes, and of unfavorable moral impressions, is so obvious that it need not be enforced by examples.

I have thought it useful to give an outline of some of the leading principles and their practical results, to which the attention of the physician should in all cases be first directed. I have not followed the example of Halliday in including certain of the narcotics among the resources of rational treatment, because it may be said that they are always indicated as palliatives, while their curative efficacy appears not in proportion to their narcotic power, but connected with some peculiar property of the particular substance employed. I have at least avoided the contradiction into which he has fallen, of ranking remedies like hyoscyamus and belladonna among the means of rational treatment, and at the same time including cicuta among the empirical agents.

It can hardly be expected that in speaking of the empirical modes of treatment, I shall enumerate all the remedies which have been employed for neuralgia. To do this would give my dissertation the aspect of an index to the Pharmacopœia. I shall therefore especially notice such as are best recommended, or

¹ Ibid. p. 134. (Observation from Journ. Comp. des Sc. Méd.)

such as are entitled from their novelty to further trial. These I shall consider under the head of *External* and *Internal* means, and conclude this essay with a view of the surgical operations which have been proposed for the relief of the affection.

EXTERNAL APPLICATIONS.

Leeches. As a remedy in ordinary uninfammatory cases, they seem to have little efficacy, as may be inferred from almost all the instances of neuralgia which have been published. In some of M. Piorry's cases they appear to have changed the character of the pains from the continued to the intermittent type.¹

The general utility of *cupping* is about as problematical as that of leeching.

Pressure and *friction* have been occasionally employed with advantage of a temporary kind. Both are used instinctively by patients, as I have had occasion to witness. Lentin had observed patients suffering from tic douloureux, rub the cheeks with such force as to produce their induration, and to wear out the linen cloths which they used for this purpose.²

Cold applications. Ice was used by Fouquet, of Montpellier, with advantage.³ A case may be found in the fourteenth volume of the London Medical Journal (p. 155), in which the pains were always relieved by the application of compresses wet with cold water.

Warm applications. I have referred to a case of inflammatory neuralgia, in which relief was obtained

¹ Obs. 6th, 11th, 14th.

² Langenbeck, Tract. cit. p. 42.

³ Boyer, Traité des Mal. Chir. Vol. VI, p. 350.

by keeping the arm immersed in the warm bath. According to M. Jolly, baths, emollient cataplasms, oily and mucilaginous liniments generally fail of producing benefit. In General Dearborn's most interesting history of his own case of sciatica,¹ the warm bath at the temperature of 100° was used for eight or ten nights, but "the effect was rather deleterious than salutary." Baths of warm sand were advised by some of the ancient writers, and a kind of dry douche, consisting of heated gravel stones, by Pouteau.² M. Biett is in the habit of prescribing vapor baths, or aromatic fumigations in the cases of sciatica, &c., which present themselves at his consultations.

Rubefacients. I have found relief follow the application of a warm mustard poultice in an unusually severe paroxysm of pain, seeming to be seated in one of the cutaneous branches of the sciatic nerve, where it had occasionally appeared for many years. The remedy of this class now attracting most attention is *Veratria*. This is highly recommended by Dr. Turnbull, of London, in the form of ointment, and in the proportion of from fifteen to forty grains to the ounce of lard, of which a portion of the size of a nut may be rubbed on the skin daily.³ Mr. Liston considers both this substance and *aconitine*, as deserving trial in the neuralgic pains following amputation.⁴ In a case mentioned by Mr. Brodie, in the lecture to which I have before referred, the veratria was prescribed in the proportion of a scruple to an ounce of lard, but in con-

¹ N. E. Med. and Surg. Journal, Vol. X, p. 112.

² Monfalcon, Art. cit.

³ Wood and Bache's Dispensatory, p. 668, 3d edit.

⁴ Lect. cit.

junction with other remedies, so that it does not appear that the cure was owing to this substance. He speaks of the remedy as having been lately brought forward as possessing extraordinary powers over a number of diseases, neuralgic affections among the rest. In one patient, it seemed to him to afford relief to pain in the forehead, connected with disease of the frontal bone, but he was not satisfied that this relief was due to the ointment. In several other cases he had recourse to it, without the smallest advantage. One of Sir Charles Bell's patients, "had swallowed pounds of iron, and used veratria externally without relief, before she applied to him."¹

Blisters. They were highly recommended by Crotti, but according to Monfalcon, as a consequence of his theory of the nature of the disease. He chose for the place of their application, those parts where the nerve was most superficial. Five cases are spoken of by him, in which they relieved painful affections of the upper extremity (cubital neuralgia, according to Monfalcon). But the latter author considers them as one of the last remedies to be resorted to, and remarks that they have failed in a great number of cases, and considerably aggravated the pain in many others. He had previously spoken, however, in the same article, of an instance in which the application of a blister, by the direction of Bichat, was followed by the entire cessation of the disease. Langenbeck quotes an instance or two of their utility, but the author, from whom they are borrowed, directs them to be applied near, and not upon the affected part, since, in the lat-

¹ Lect. in Lond. Med. Gazette.

ter case, they are apt to render the pain still more intolerable.¹ Dr. Macculloch has always found the effects of blistering mischievous in sciatica.² M. Piorry speaks of them more favorably; he would have them made narrow and long, and applied along the track of the nerve.³ After all, as they are almost always tried, we should have more instances of their success, were not this very insignificant.

Endermic treatment. M.M. Lesieur and Lembert introduced the method of applying different remedies upon the surfaces denuded by vesication.⁴ In this manner narcotics, antispasmodics, &c., have been presented to the cutaneous surface with advantage. M. Piorry recommends, in case blistering does not alone prove sufficient, the application of the acetate, and especially the hydrochlorate of morphia, of stramonium, belladonna, and hyoscyamus. He has sometimes succeeded by the use of these means, but at other times they have failed.

In an account of experiments upon the effects of the endermic method published at Berlin in 1835, and reviewed in the *Encyclographie*, published at Brussels,⁵ it is said, that quinine, applied in this manner, has proved useful in intermittent fever and in neuralgia, and that morphia has been sometimes advantageous in sciatica.

M. Magistel has published a memoir on the employment of the acetate of morphine in the treatment of certain neuralgiæ, by the endermic method.⁶ The

¹ Op. cit. p. 44.

² Essay on Marsh Fever, &c. p. 273.

³ Op. cit. p. 290.

⁴ Jolly, Art. cit.

⁵ 2d Series, Vol. VI, p. 99.

⁶ Gaz. Méd. de Paris. Reviewed in Journ. des Connaiss. Méd. Chir. March, 1835.

reviewer of this article, remarks on the impropriety of confounding hemicrania and neuralgia, as M. Magistel has done ; but at the same time he considers his conclusions valid with regard to neuralgia, since all the cases, which he has cited, belong to this affection. The following are the two last of M. Magistel's conclusions.

The local action of narcotics constitutes the best mode of treatment even in symptomatic *hemicrania*.

The acetate of morphia employed upon the dermis denuded by the ammoniacal ointment is the most useful therapeutic agent in *hemicrania* as well as in many other neuralgiæ.

On this the reviewer remarks, "Certainly the treatment which he recommends is one of the most powerful in affections of this kind. Perhaps no one has seen this treatment employed more extensively than ourselves ; and we can say that facial neuragia rarely resists it. M. Magistel uses the acetate of morphia. It is a long time since M. Trousseau, convinced of the uncertainty of this almost insoluble preparation, substituted the sulphate, or the hydrochlorate with great advantage. Thus if the conclusions of M. Magistel be applied to the treatment of facial neuralgia, they will prove very true and very useful ; but they must not be extended to that of hemicrania, where they would be found false and injurious."

A few months after, the same writer, in reviewing a paper of M. Mondière, returns to this interesting subject.

"M. Mondière relates a case of sciatica, which had resisted several kinds of treatment, and which readily yielded to the acetate of morphine administered in the

endermic method. I have already spoken in the March number, of the preference which should be given in these cases to the sulphate and hydrochlorate, and the reasons for it; I repeat it, notwithstanding the success obtained in this case by M. Mondière, because I have seen the comparative results in considerable number, and because this practitioner, having only employed the acetate, is not aware of the comparative powers of the two other salts. M. Mondière makes use of cantharides to denude the dermis. I would point out two objections to this process; the first, that of leaving the patient for twenty-four hours longer under his sufferings (*fulgura doloris*), while with the ammoniacal ointment well prepared (at 22°, at least), in three or four minutes we can obtain a surface capable of absorbing the narcotic; the second, that it is impossible to watch the vesicating action of the cantharides, and that the mucous body of the skin is almost always acted upon and inflamed, and we know that the inflammatory process which is thus established, is more or less unfavorable to absorption. The treatment employed here is sanctioned by many analogous instances of success; but I am far from thinking with M. Mondière, that it is competent to the radical cure of sciatica. I have rarely seen it sufficient without energetic and long continued auxiliaries. Cinchona in large doses, the pills of Meglin, moxas and vapor douches require this means before they can become useful; but alone, and without their assistance the latter rarely effects a complete cure.”¹

Bellingeri prefers the external application of the

¹ Journ. cit. May, 1835.

acetate of morphia dissolved in the oil of sweet almonds, on the denuded or sound skin, to its internal application. But he mentions the very important fact, that two grains of this substance, employed after a blister upon the lumbar region, occasioned the symptoms of poisoning. He uses some other narcotics with more boldness.¹

Setons and Issues. Forstmann has published a case, in which the suppuration of a wound was accompanied by the relief of tic douloureux, which reappeared upon its cicatrization, and again ceased when suppuration was renewed.² The author from whom this instance is taken, (Monfalcon) asserts, that exutories have been useful in some circumstances, but how often, he exclaims, have they been tried in vain! Boyer says, that he has repeatedly applied blisters, setons and moxas to the nucha without any advantage, in tic douloureux. M. Jolly does not consider these means as more to be depended upon than the other direct revulsive stimulants.³

Moxa. We have seen the opinions of Boyer and Jolly upon its efficacy. Hamel had seen this remedy applied upon the diseased part without any effect.⁴ Larrey, the great advocate of this means, employed it successfully in three cases of tic douloureux, but Halliday remarks that the professional brethren of this celebrated surgeon have not always found the same wonderful effects follow it in their practice, which he met with in his own.

¹ Review cited of Art. in *Annali Universali*, &c.

² Monfalcon, Art. cit.

³ Dict. de Méd. et Chir. prat.

⁴ De la Névralgie faciale, p. 23.

Acupuncture. M. Jules Cloquet, who published an essay on this method of treatment, obtained favorable effects, and even speedy cures from it in muscular rheumatism, in facial, dental, suborbital, cubital and sciatic neuralgia.¹ But this remedy has gone nearly out of fashion. Halliday allows that it has cured some cases, and rendered others more supportable ; and would have recourse to it when other agents have failed. In the *Journal des Connaissances Medico-Chirurgicales* for June, 1834, is a notice of a memoir by Dr. Narducci, in the *Osservatore Medico*, upon the efficacy of acupuncture. One of the cases given by the Italian physician is of pain in the arm of an uncertain character, another is a case considered as sciatica. Besides these cases, in which the effect was rapid and remarkable, he adduces five others of the same kind, but less wonderful. He makes use of needles with an eye, through which a thread is passed, to prevent their getting beyond reach, and removes them when the pain is relieved, and the patient unable to support the pricking which they occasion.

Electricity. This agent is little spoken of by the recent writers. More than thirty years ago, Hamel recorded the following conclusions. 1st. That electrization by means of the bath produces no effect. 2d. That by means of shocks it produces only a kind of stupefaction which suspends the pain for a very short time. 3d. That by means of sparks taken from the part affected, it becomes a stimulus adapted to increase the action of the skin, and which may be

¹ Dict. de Méd. et Chir. prat. Art. Acupuncture.

efficacious if it produce an eruption followed by a serous discharge.¹

And more recently, Dr. Macculloch uses these words, in speaking of the remedial power of the electric shock. "While my own attempts, pursued on a very great scale for very many years in a military hospital, and for the direct purpose of ascertaining the value of this remedy, have had no success whatever, I cannot find that others have been more fortunate; though I can easily imagine that such a disease might thus be cured through the influence of the imagination."²

Electro-puncture has been considered a useful combination of the two means last mentioned. Halliday knew it to have aggravated the pains considerably in a case of facial neuralgia.

Galvanism. "Narless, Leydig and Grapehgiesser have made trials of galvanism which were not favorable. Ritter, Quen and Chisholm were more fortunate."³ In the American Journal of the Medical Sciences for August, 1834, Dr. Harris has given an account of several cases treated by Galvanism applied by the method of Mansford. For the description of this ingenious contrivance I refer to the paper mentioned. Having succeeded in one case, Dr. Harris tried this remedy in seven others. Four of them are said to have been cured, the first after eleven days, the second after twenty-three, the third on the tenth day, the fourth after five weeks. In the case of

¹ *De la Névralgie faciale*, p. 25.

² *Essay on Marsh Fever and Neuralgia*, p. 420.

³ Halliday, *Op. cit.* p. 164.

Gen. Dearborn, before referred to, the patient found no benefit, although it was applied night and morning for five days, half an hour each time, from a trough of fifty plates, each four inches square.

Magnetism. The conclusions of Andry and Thouret, arrived at more than half a century ago, are adopted by Halliday. That of Thouret may be thus rendered. The magnet is of real, though slight benefit. It acts only as a palliative, but in such an affection no means of relief, however trifling, should be neglected. In Hufeland's Journal for July, 1834, is the history of a case in which its employment was followed by momentary relief of neuralgic pains, and which may be recommended to the lovers of the marvellous for the strange phenomena said to have taken place under its action.¹ M. Bulmerincg (Fragments on the effects of mineral Magnetism. Berlin, 1835) has observed salutary effects from the application of the magnet in many diseases, and above all in neuralgia of every kind. He applies two horseshoe magnets to the lower extremity of the body (to the calves in preference) as did Mesmer and Unzer, and a third in the form of bar, horseshoe, or disk to the affected part.²

"With regard to animal magnetism," with the learned and sensible Halliday, "we will say for those who believe in it, that it has been often known to calm the most violent paroxysms, but rarely to cure the disease, and for those who do not believe in it, that we do not stand sponsors for the reality of its marvellous doings."

¹ See Journ. des Connaiss. Méd. Chir. for Dec. 1834.

² Encyclographie, 2d series, Vol. VI, p. 98.

I will only add, that among the external remedies, sea bathing and the use of medicinal baths are spoken favorably of by Jolly and Halliday.

INTERNAL REMEDIES.

Narcotics. Among this class no one appears to have as high claims as *Conium*. Dr. J. Fothergill, who found it afford relief in a case which he mentions, always had recourse to it afterwards, and for the most part with success.¹ In 1813 this article was again brought before the public by Dr. Jackson, of Boston.² He observes that the character of the medicine had very much depreciated since the time of Dr. Fothergill, which he attributed to the bad quality of the article employed, or to the timidity or want of attention of the physician. He declares that his own experience of its efficacy accords with that of Fothergill, and details three cases illustrative of its utility. Dr. Haighton, who published his famous Case in the same year (1813), declares that this remedy has not been so successfully employed by others as they were led to expect from the high recommendation of Dr. Fothergill.³ “*Len- tinus vero,*” says Langenbeck, “*electuarium Baldingeri e cicuta fæminæ cuidam adhibuit, quo quidem hujus dolores ita leniebantur, ut post usum ejus duos menses continuatum, quatuor annos doloribus libera esset, tandemque sulphure aurato, et extracto cicutæ sanitati restitueretur.*”⁴ Underwood, an English surgeon, says Hamel, reports that he was cured

¹ Med. Obs. and Enq. Vol. V.

² N. E. Med. & Surg. Journal, Vol. II.

³ Vide Med. Rec. and Researches, p. 50.

⁴ Tract. cit. p. 43.

by the use of this remedy ; a fact for which he refers to the *Traité des Ulcères* published at Paris, 1784.¹ In the American edition of Dr. Gregory's practice of Physic, cases are mentioned from Dr. Thacher, and from the N. E. Medical Journal, in one of which the disease seems to have been mitigated, and in the other cured by this remedy.²

In Dr. Gorrie's dissertation,³ before referred to, I find the following testimony regarding conium. "Dr. Willoughby, the distinguished professor of obstetrics in the University of the State of New York, informs me he has given it in several cases with great benefit. Much more evidence, and some of it quite recent, could be adduced to prove the service of cicuta in neuralgia. It must not, however, be concealed that it has often failed ; enormous quantities of the extract, obtained from respectable druggists, have been taken in a short space of time without procuring even momentary relief." Referring to the manner in which Dr. Jackson accounted for its failure, he remarks, "A more probable cause of its frequent failure is its too general use. From the evidence on the subject it appears that no one of the class of narcotics is more deserving of our confidence than a good preparation of cicuta."

The following remarks are from the lecture of Mr. Lawrence,⁴ previously cited. "Of these (the narcotics) I must observe, according to my own experience, that the only one on which we can have any reliance for checking the paroxysms, is conium. I

¹ De la Névralgie fac. p. 32.

² Vol. II, p. 163.

³ N. Y. Med. and Phys. Journal for 1828.

⁴ Lond. Med. Gaz. Vol. VI, p. 643.

have seen in several cases, where it has been given largely and at short intervals—and it must be given in such doses as to produce some of its peculiar effects on the nervous system—that it has put a stop to the paroxysms, and for such a length of time that I have thought it has cured the disease; but in some instances, where persons have remained well for several months, and even, in one case, for more than a year, the pains have again showed themselves, but the agony has been considerably controlled by it. As means of a narcotic kind, for controlling the disease, I therefore place more reliance on conium than on any other of that class.”

The following is the summary of Halliday. “Fothergill, who saw in these diseases only a disguised cancerous affection, regarded the cicuta as the only means to be relied upon. Selle took the same principle as his starting point, and professed the same opinion on the excellence of the remedy. Lentin never saw it effect a complete cure in tic douloureux, but he believed it a proper introduction to the treatment, and favorable to the action of further remedies. Pujol, Jackson, Gessner, Thileney, Jalhn, have seen it effect cures; Siebold and Masius have found it produce much relief; it has completely failed in the hands of Haighton, Schlegel, Blunt, Keup, Reil, &c.”

Stramonium. The name of Dr. Marcet has given a certain reputation to this remedy, which, however, is much less strongly recommended than the preceding. Dr. Marcet reports fourteen cases, and draws these conclusions. “Thus, from the facts I have just laid before the Society (the only ones that have yet come under my own observation respecting the effects

of stramonium), it would appear that in four cases of sciatica, decided benefit was obtained. The efficacy of this remedy was still more strongly marked in two cases of sciatica combined with syphilitic pains. It failed entirely in two instances of diseased hip joint. It produced considerable relief as to pain in a case of supposed disease of the spine followed by paraplegia, and likewise in one of cancer of the breast. It allayed materially the pain occasioned by an acute uterine disease. It was of great and repeated utility in a case of *tic douloureux*; its utility in a second case of the same description was very doubtful; and in a third it entirely failed.”¹

Some other cases of its utility have been published, but it has not on the whole obtained much evidence in its favor.

Belladonna and *Aconite* have found their advocates, but have inferior claims to some of the other narcotics.

Hyoscyamus. The form in which this remedy has attracted most attention is in the combination constituting the pills of M^églin. I derive from Monfalcon the account of their success which follows. Nine circumstantial observations in which they effected a cure have been published by M^églin. M. Louis Valentin employed them once without success, because they were rejected by vomiting; in another of his cases they entirely succeeded. A controversy arose between the inventor and M. Chamberet; the latter pretending that M^églin lost sight of his patients too early, and doubting if the cures were radical;

¹ Med. Chir. Trans. Vol. VII, p. 551.

and the inventor stoutly defending his pills, declaring that he had learned all the necessary circumstances, and that the cures were brought forward as radical. "As to the rest," says Monfalcon, "he does not propose his pills as a specific ; but he says, and it seems to me on sufficient grounds, that the frequent instances of success obtained by his method, inspire us with reasonable hopes of increasing their number, especially if the cases be not too inveterate."

Some others, according to Halliday, have obtained favorable results from the use of hyoscyamus, either alone or in combination ; others have found them of no avail.

Opium. The general testimony of observers is by no means as favorable as might have been anticipated to the powers of this medicine. Halliday, who speaks of the infinite number of physicians who have complained of its want of efficacy in facial neuralgia, supposes it may be owing to its having been employed so much oftener than other remedies, in a disease beyond the resources of medicine in the majority of cases. He remarks that its too long continued use has been sometimes known to aggravate the affection considerably.

Two remedies which have each high claims to attention remain to be noticed. The first of these is the *subcarbonate of iron*. Mr. Hutchinson, who brought it especially into notice, gives twenty-seven cases, in all but two or three of which it was successful. He has known it to fail in a few others which he has not published ; he attributes this to a want of perseverance in its employment.¹ Even in

¹ Cases of Neuralgia Spasmodica, &c. London, 1822.

the very few instances where he allows that it has failed to cure the disease, he asserts that it has done more to alleviate it than any known remedy.¹ Dr. Elliotson published a paper in the 13th volume of the *Medico-Chir. Transactions* (1827), confirming the opinion of its virtues. He remarks in his essay on Neuralgia in the *London Cyclopædia*, that it may be used in very large quantities; as children of eight years old will often take half an ounce, or six drachms every four hours. Strict attention should be paid to keep the bowels open, and if it fail in doses of a drachm every six hours, the larger quantities should be exhibited. Although he allows that it frequently fails altogether, and that still more frequently the success is only temporary, yet he considers it the best remedy when there is no inflammation or evident existing cause.

In the same year in which Dr. Elliotson first published his results (1827), Dr. Duparque brought forward a number of cases successfully treated by the same medicine in the *Nouvelle Bibliothèque Médicale*. The editor of an *American Journal*,² in noticing this paper, remarks that notwithstanding the testimony of French and English authors in favor of this method of treatment, the medicine has been found wholly inert in this country. "For ourselves," he adds, "we have never seen it productive of the slightest benefit." This remedy is, however, recommended by Dr. Chapman of Philadelphia,³ and a case is reported in

¹ Cases of Neuralgia Spasmodica, p. 184.

² *Phil Journ. of Med. and Phys. Sciences*, Vol. XIV, p. 153.

³ *Am. Journ. of Med. Sciences*, August, 1834.

the N. Y. Medical and Physical Journal,¹ in which neuralgia of twelve years standing yielded to it. Sir Astley Cooper speaks of it as the best remedy in tic douloureux;² but notwithstanding the praise it has received, a critic in the Medical Intelligencer has attacked the evidence in its favor as unsatisfactory.³ M. Roche prefers it to other means, and has rarely found it fail. But his doses are only from twenty-four to forty-eight grains in the day.⁴ To close once more with the testimony of Halliday, "Wittke obtained the most favorable results from this medicinal agent. He gives it in the dose of a scruple with five grains of canella three times a day. (Hufeland. Journ. 1828, T. 1). The English Journals abound in instances of its success; it would be superfluous to adduce them here. Nesse Hill, on the contrary, found no advantage from it; Masius was not more fortunate, and we ourselves have seen it completely fail, but in a case, it is true, little favorable to it, since the patient's stomach could only support very small doses." A remarkable instance of its efficacy is referred to in the article Tic Douloureux of Cooper's Surgical Dictionary.

Turpentine. This substance was first used internally by Cheyne, and with advantage, as it was afterwards by Home.⁵ It succeeded several times in the hands of M. Recamier.⁶ In the year 1823, M. Martinet published a memoir on the use of oil of turpen-

¹ Vol. III, p. 123.

² Castle's Manual, p. 308.—Lectures, &c. p. 439.

³ Vide Cooper's Surgical Dict. Art. Tic. Doloureux.

⁴ Nouveaux Éléments, &c. Vol. II, p. 359.

⁵ Monfalcon, Art. cit.

⁶ Ibid.

tine in sciatica in the *Revue Médicale*, of which I find the following summary in the *N. Y. Medical and Physical Journal*.¹

“Of seventy patients who labored under sciatica, or other severe neuralgiæ of the extremities, fifty-eight were cured; namely, three by frictions, and fifty-five by the internal administration of the turpentine. Seven experienced only a more or less durable relief, and five derived no benefit whatever from the medicine. Two out of these last labored under disease of the hip joint, under which they ultimately sank. Of these seventy cases, forty were acute, and thirty chronic. Of the forty acute cases, thirty-four were cured, five were relieved, and one received no benefit. Of the thirty chronic cases, twenty-four were cured, two were relieved, and four remained without any mitigation of their complaints. Of the fifty-eight patients who were completely cured, thirty-four were cured in less than six days, twenty-two within twelve days, and three within six weeks. Of these fifty-eight cures, forty-eight were cases of sciatica, two of which were cured by frictions. Three were crural neuralgiæ, four were brachial, and three facial. In the ten cases which were only relieved (all of which were cases of sciatica), the treatment was suspended after the second day. In twenty-one cases heat was felt along the tract of the nerve, and in the limb affected; and nineteen of these were completely cured. In eighteen cases, heat was felt in the stomach and bowels. Three were affected with vomiting, but these had taken a larger dose of the oil than usual. Three had

¹ Vol. I, New Series, p. 476.

diarrhœa and colic. In five instances, the urine was augmented—four had dysury or strangury. In ten instances the perspiration was augmented. In one case a degree of intoxication was produced—and in two others, a pruritus was experienced over the body.”

In the *Revue Médicale*, for 1824, M. Dufaur has published seven cases of neuralgia, cured by the oil of turpentine. Six of these were affections of the sciatic, and one of the brachial nerve. From these observations, he derives these results.

1. That of seven patients, affected with sciatic neuralgia for the most part, six were cured in the space of a few days.

2. That these six patients, five of whom, in fact, had been but a short time affected, had tried different methods of treatment without any success.

3. That the only patient who was not cured, had been affected with sciatica for several years, and had employed different remedial agents without advantage.

4. Finally, that the oil of turpentine, in these different patients, did not act either as a diuretic, a purgative, or a sudorific.

Mr. Lawrence speaks of oil of turpentine in connection with mercury, arsenic, and bark, as having been tried in all quantities without good effect;¹ but their occasional or frequent failure does not prove, that each of them may not often have been useful. M. Roche remarks, that this remedy, whose method of action is little understood, has appeared to

¹ Lect. in Lond. Med. Gaz. Vol. VI, p. 643.

him to possess considerable efficacy in some of the neuralgiæ.¹

Although I have been obliged, in order to avoid repetition, to speak of the remedies used in neuralgia of different parts under one general head, yet certain of these remedies have been considered peculiarly adapted to neuralgia of one part, and others to the same affection in another situation. I will repeat the list of these medicinal agents and their application, after M. Jolly.²

In facial neuralgia have been particularly recommended, the pills of Meglin, the extracts of valerian, of assafœtida, of belladonna, the chloruret of zinc, the preparations of cinchona, of iron, etc.; in neuralgia of the limbs, applications of ice, cataplasms of belladonna, blisters, moxas, vapor baths, sulphurous and aromatic baths, turpentine, frictions with Pearson's solution, with the tartar emetic ointment (pommade d'Autenreith), with that of Cirillo, circular ligatures, etc.; in lumbar neuralgia, enemata of cyanuret of potassium, cupping, blisters, &c.; in internal or ganglionic neuralgia, valerian, opium, ether, castor, musk, assafœtida, subcarbonate of iron, gaseous waters, etc.

The most approved formulæ, as given by this writer, will be subjoined at the close of the essay.

SURGICAL OPERATIONS PROPOSED FOR THE RELIEF OF NEURALGIA.

Galen, as is mentioned by several writers, had already spoken of the *section* of the nerves, in an

¹ Nouveaux Éléments, &c. Vol. II, p. 360.

² Dict. de Méd. et Chir. prat. Art. Névralgie.

affection attended with delirium and spasm. Nuck conceived the idea of paralyzing the nerve by section; but, says Monfalcon, for the cure of toothache by this method, he looked for the nerve on the anti-tragus, which takes off a little from the merit of his idea. A surgeon of my acquaintance has very frequently performed this operation at the request of patients, who had from some source or other acquired a conviction of its efficacy, and I believe it was found profitable to both patient and operator. Section of the nerves was subsequently performed by several French surgeons, but was especially brought into notice by the well known case published by Dr. Haighton. In this instance, the patient had remained free from pain for nine years from the time of the operation, when Dr. Haighton published the account. But it has been long observed, that the pain was very apt to return in a short time; commonly in a few weeks, and sometimes even in a few days, according to Hamel;¹ a natural consequence of the union which has been shown to take place in divided nerves.

It was this consideration which led André to the employment of *caustic* upon the affected nerve; in addition to which he kept up suppuration in the wound long after the pain had ceased. By means like these, says Hamel, he obtained success in cases, where the simple section of the nerve had been ineffectual.

The same motive suggested the *excision* of a part of the nerve, the operation most generally approved at the present day.

¹ De la Név. Fac. p. 27.

The use of caustic is, however, considered preferable to excision by Boyer, who draws the following parallel between them, with his usual admirable clearness and simplicity.

“Cauterization in the method of André, has the inconvenience of producing much more deformity than that which succeeds the ablation of a part of the nerve ; but it also offers more chances of success ; it destroys not only the whole substance of the nerve for a certain distance, but it also attacks all the nervous branches proceeding from it through a considerable extent, which, as they may participate in the disease, might be sufficient to keep up the pain, after the resection of the principal trunk. It is not reasoning alone which leads me to give the preference to cauterization ; experience justifies this preference, and to be convinced of it, it will be enough to compare the results obtained by André, to those witnessed every day from the other method.

“The latter (excision) has also another inconvenience, which is the extreme difficulty of its execution. I know that this objection is generally treated as of very slight, or no consequence, and that it is supposed the difficulty vanishes in skilful hands ; but the best trained surgeons are much embarrassed in executing this operation, and often finish it without being sure of having removed the diseased nerve. I have myself performed it upon the frontal nerve, at its exit from the superior orbital foramen, for a supra-orbital neuralgia, from which a man, forty-three years of age, had suffered for several years. The pain was suspended for some months ; but then returned with its former intensity. M. Roux performed the same oper-

ation at La Charité, three different times, on a young man, twenty-three years of age, first on the mental nerve, then on the suborbital, and subsequently on the portio dura of the seventh pair. The first of these operations procured a respite of three months; the second did not even suspend the pains, and the relief which followed the third ceased when the wound had cicatrized. In the two last instances, it could not be ascertained with absolute certainty, that the nerve had been divided; it was supposed to be detected in the small portion of soft parts removed; but there remained some doubts on this point, which it was impossible to clear up with all the attention that could be given."¹

Dr. Mott, of New York, prefers insulating a portion of the nerve, by repeated incisions through its substance at a small distance from each other, to the removing a portion of it.²

Dr. Mitchell states, that he has cured neuralgic pains of the scalp, by tying the branch of the temporal artery going to the part affected, according to a suggestion of Dr. Mott.³ Dr. Parry had already suggested that the success in Dr. Haighton's case was owing rather to the division of the arterial branch supplying the ramifications of the trigemini, than of the nerve itself.

It remains to appreciate briefly the comparative utility of these different operations.

1. It is certain that the simple division of the nerve

¹ *Traité des Mal. Chir.* Tom. VI, pp. 351, etc.

² *Cooper's Surg. Dict.—Additions of American Editor.*

³ *Gregory's Elements, &c.*, Vol. II. p. 165 (Addit. of Am. Editors).

may effect a cure. (Observation of Haighton; case related by Dr. Darwin;)¹ case by Leydig, given in Halliday;² But it is generally agreed upon that this operation is frequently useless, or produces only temporary advantage, as may be seen by referring to the authorities of Monfalcon, Boyer, Sir Astley Cooper, Mr. Abernethy, Mr. Brodie, Mr. Lawrence, and Sir Charles Bell. Or if instances of nearer origin are required by those who read this essay, I may cite the two first cases among those given by Dr. Warren, in the Boston Medical and Surgical Journal for February, 1828.

I am not aware that others have repeated the experiment of Dr. Mott, and the operation practised by Dr. Mitchell, besides standing on limited authority, appears less rational than those which act directly on the affected nerve.

But of excision and cauterization, we have many instances of success, while, if there have been failures, they do not at least meet us at every step, like those of simple section of the nerve. In favor of the former, we have the confident and decided testimony of Dr. Mott,³ to which I could easily add that to be derived from many particular accounts of cases. For the latter, we have the arguments and experience of Boyer previously cited.

I believe that each of these operations may, at different times be preferable, but that to one of them, whenever practicable, the preference should be given over the other methods. Excision should be pre-

¹ *Zoonomia*. Vide Good, Vol. IV, p. 218, &c.

² *Op. cit.* Obs. 14th.

³ *Surg. Dict. Art. Tic Doul.*

ferred, when the nerve is of sufficient size to be easily detected by the scalpel; when it is important to avoid the deformity of a cicatrix; and above all when the operator is qualified by his exact knowledge, patience, habit, and manual dexterity for one of the most delicate and perplexing tasks in the province of surgery. In the opposite circumstances, I would not hesitate to adopt the plan of cauterization. That instances of want of success, that occasional relapses, that difficulties attending the operations for the relief of neuralgia, should be made a ground for condemning this operation, appears to me, to say the least, unreasonable. We are dealing with an affection, which, as one of the Committee to whom this essay is subjected, has said, is probably the most distressing of disorders. The sufferings it occasions, have sometimes closed only with life. In different cases, whose history is before me, patients have, as it were, grown mad with pain, or writhed upon the floor in uncontrollable agony, or been racked with convulsions that led to apprehensions of tetanus; they have longed for death, they have meditated, nay, committed suicide. They have been willing to undergo repeatedly the most dreaded operations; even to suffer amputation at the articulations of the hip and the shoulder. If all other means have failed, even if we hoped only for a respite from all this misery, would not a few months of ease be cheaply purchased with a few minutes of pain, not to be compared with that of the original malady? Still more evidently, then, since the operations practised have often cured the disease, it is the imperative duty of the surgeon to resort to this *ultima ratio* of baffled, but not defeated science.

"They lift up their eyes, being in torments"—and the drop of cold water must not be withheld from those who seem to suffer on earth, all that legends and fables have banished to the regions of despair.

I have only to indicate the nerves upon which the different operations mentioned have been performed.

The three principal branches of the fifth pair have been much oftener subjected to it than any others. If the operation be admissible, there can be no doubt that to these it is properly applied. With a single exception, the three branches have been always divided after their exit from their respective foramina. Dr. Warren, in a severe and long continued case, divided the inferior maxillary branch before it enters its canal; arriving at it in this situation by the application of the trephine.¹ I will not conceal the fact that Halliday has qualified this with the term of an *extravagant* operation. Nor will I withhold my own opinion, that the best answer, and a satisfactory one to this remark, is to be found in the result of the case in question, so far as its history is recorded.

The operation upon the facial nerve (*portio dura*) is absolutely proscribed by Halliday. The experiments of Sir Charles Bell, showing that paralysis of certain muscles is produced by dividing this nerve are well known. In one of his recent lectures, which I have previously cited, he seems to consider this operation as entirely irrational.² I have already referred to cases seeming to prove that this nerve may be the seat of neuralgia. Since the time of Marechal,

¹ Vide Boston Med. and Surg. Journal, Vol. I, No. 1.

² Lond. Med. Gaz. Vol. XVII, p. 874.

according to M. Blandin, the section or excision of a part of the facial nerve has been performed from time to time, especially by Roux, but almost always without success. This he thinks may perhaps be attributed to the fact that the operation has generally been performed in the region anterior to the parotid, where the nerve is already divided, and its filaments are scattered apart. On this account Beclard proposed to divide the nerve at the point of its exit from the stylo-mastoid foramen.¹ This was the operation performed by Dr. Warren in the same case in which he afterwards cut out a portion of the inferior maxillary branch above its entrance into the canal; here also he removed a portion of the nerve. Although this plan was suggested by Beclard, I do not know that it was ever performed before the case just alluded to. Its execution was followed in this instance by paralysis, but by none of the dangerous accidents to which Mr. Swan supposed it liable. It was also followed by a limited degree of relief. In the case given by Dr. Good, from the *Zoonomia*, partial benefit resulted from the division of some of the branches of the portio dura; but the evidence I have adduced must leave the general expediency of the operation somewhat doubtful.

The instances in which division of the nerves of the extremities has been performed, are comparatively rare. I have met with the following; others might probably be added.

Mr. Abernethy removed three quarters of an inch

¹ Blandin, *Traité d'Anat. Topograph.* Paris, 1834. p. 189.

in the course of the nerve of one of the fingers with ultimate, but not immediate relief.¹

Dr. Schott, of Frankfort, removed an inch of the cubital nerve above the inner condyle of the humerus, for a severe neuralgia of the ring finger, of fourteen years standing. The pain ceased, but after three months returned; not so severe, however, as before the operation.²

Finally, M. Malagodi excised a portion of the sciatic nerve about eighteen lines in length, a little above the popliteal cavity. The pain ceased entirely, and was replaced by paralysis of the leg and foot, with a sense of weight and formication, and impaired sensibility at the inner surface of the leg.³

From all that precedes, I deduce the following conclusions in answer to the questions proposed.

1. The nature of neuralgia, cannot be absolutely explained so long as we are ignorant of the mechanism of sensation in the healthy condition.

2. Neuralgia is sometimes the consequence of appreciable changes in the course of the nerve affected; at other times no changes can be demonstrated.

3. It is sometimes a manifestation of disease in another organ more or less remote.

4. It is a symptom by which the nerves give evidence of their participation in certain diseases wandering, or metastatic in their nature.

¹ Lect. on Surgery. London, 1831, p. 125.

² Tiedmann on Regen. of Nerves. Lond. Med. Gaz. Vol. X, p. 680.

³ Malgaigne, Manuel de Méd. Op. Paris, 1834, p. 155.

5. It is a result in some cases of the condition induced by malaria.

6. It is often without obvious cause.

7. The best method of treatment is that which removes any known cause, and modifies any ascertained morbid condition.

8. The empirical remedies whose utility is best established, are the subcarbonate of iron, the oil of turpentine, and cicuta. The endermic application of narcotics is perhaps most deserving of future experiments.

9. The operation by excision or cauterization should be resorted to in case of the failure of milder remedies.

I subjoin the formulæ given by M. Jolly at the end of his article on Neuralgia in the *Dictionnaire de Médecine et de Chirurgie Pratiques*. They consist of the most popular remedies used in France, and the names attached to them will be enough to give them interest. For the details referring to other medical agents, and the different surgical operations mentioned, I must refer to the treatises on each of these general subjects.

Bolus used in Intermittent Neuralgia. (Chaussier.)

℞. Cinchonæ, ʒiv.

Ammon. Muriat. ʒss.

Syrup. fol. Amygd. Pers. q. s.

Ft. bol. viii.—One every three hours.

Enema used in Lumbar Neuralgia. (Martinet.)

℞. Ol. Terebinth. ʒss.

Vitell. Ovi. no. j.

Decoct. flor. papav. lb. ss.

M. To be used during the remission or intermission.

Liniment for Neuralgia of the extremities. (Martinet.)

- ℞. Ol. Anthem. ℥ij.
 Ol. Terebinth. ℥j.
 Laudan. Sydenhami ℥j.
 M. To be used in frictions.

Another Liniment. (Pearson.)

- ℞. Ol. Olivæ, ℥ij.
 Ol. Terebinth, ℥iss.
 M. Acid. Sulph. ℥j.

Lohock used in Femoro-popliteal Neuralgia. (Martinet.)

- ℞. Ov. Vitell. no. j.
 Spirit. Terebinth. ℥iij.
 Syrup. Menth. ℥ij.
 " Aurant.
 " Æther. āā ℥j.
 M. Three tea-spoonfuls in the day.

Anti Neuralgic Opiate. (Jolly.)

- ℞. Ferri Subcarb. ℥ss.
 Quinin. Sulph. gr. xvj.
 Extract. Thebaic. gr. ij.
 M. Divid. in partes xvj.—Four in the day.

Pills used in facial Neuralgia. (Méglin.)

- ℞. Extract. Hyoscyam. nig.
 Rad. Valerian. Sylvest.
 Zinci Oxyd. albi āā ℥j.
 M. Ft. pil. gran. iij.—One or two every three hours.

Other pills for the same affection. (Recamier.)

- ℞. Opii,
 Ipecacuan. āā. gr. iij.
 Camphoræ,
 Ammon. Carb. āā. gr. xij.
 M. Ft. pil. no. xxx.—One every three hours.

Other pills. (Recamier.)

- ℞. Antimon. tart.
Ext. Op. gummos. āā. gr. x.
M. Ft. pil. no. xxiv.—Two or three in the day.

Pills used in Pulmonary Neuralgia. (Bally.)

- ℞. Potass. Cyanuret.
Amyli solidif. cum syrup. gummos. āā. gr. ss.
M. Ft. pil. no. j.—One pill morning and evening.

Pills used in Gastralgia and facial Neuralgia. (Jolly.)

- ℞. Ferri Hydrocyan. gr. xviii.
Quinin. Sulph. gr. xij.
Extract. Thebaic. gr. j.
Conserv. Ros. q. s.
M. Ft. pil. no. xij.—One every three hours.

Sternutatory in facial Neuralgia.

- ℞. Cinchon. rub. pulv.
Pulv. Tabaci āā. ʒj.
M.

ON THE
UTILITY AND IMPORTANCE OF
DIRECT EXPLORATION
IN MEDICAL PRACTICE.

Inter labores et tædia.

DISSERTATION.

“HOW FAR ARE THE EXTERNAL MEANS OF EXPLORING THE CONDITION OF THE INTERNAL ORGANS, TO BE CONSIDERED USEFUL AND IMPORTANT IN MEDICAL PRACTICE?”

“The external means of exploring the condition of the internal organs,” might be construed to include all the modes of examination in which the senses of the observer are immediately applied to the organs of the patient, or to their products;—all of what have been called the objective signs. But as certain of the objective signs, the state of the pulse and of the tongue, for instance, have been long and generally allowed to be in the highest degree useful and important in medical practice, it seems expedient to depart so far from the letter of the question, as to consider only those methods of investigation which have been less extensively adopted, or whose value has been less fully appreciated. To these methods I shall give the general name of *Direct Exploration*.

To determine *how far* they are useful and important, will require much time and labor; for their importance and usefulness vary in different diseases; and it is only by a careful estimate of their value in each of these diseases, that we can form an approximative opinion of their collective value in medical practice. Our answer to the question then, must be

sought for, not in a single proposition assigning the exact degree of utility of the methods under consideration, but in a series of results relating to the most important internal diseases to which the use of direct exploration is applicable.

As each of these diseases passes before us, three questions will arise ;

1. How far are its general symptoms uncertain or insufficient to direct the practitioner ?

2. How far are its physical signs characteristic and constant ?

3. What practical results follow from the information they afford us ?

But we shall answer these questions in greater or less detail according to several circumstances. Thus, in some diseases it is so well known that the general symptoms are but imperfect guides, that no practical man requires any proof of the proposition. Again, we shall answer the second question more fully, in proportion as we may have ideas to bring forward which may appear novel or interesting. In answering the third question we shall content ourselves with indicating the most important practical consequences derived from the more accurate discrimination of various morbid conditions by means of direct exploration, without carrying them into the details which are to be sought for in systematic treatises upon disease.

We have prefixed a few general thoughts upon the subject of direct exploration, for offering which at the present day, our apology must be found in the opinions still entertained by some of those whose ideas possess a certain degree of influence with the community.

GENERAL CONSIDERATIONS ON DIRECT EXPLORATION.

The physical principles on which the different methods of this art are founded, may be expressed in the following formula.

The organs beyond our immediate inspection, are capable of producing certain changes in the form of the external parietes perceptible to the eye (inspection, mensuration); they undergo certain modifications in their consistence, volume, figure, movement, appreciable by the touch (palpation); and they can transmit to the ear, either in consequence of their own actions (auscultation), or when submitted to certain mechanical impressions (percussion, succussion), sounds which vary with their conditions. (Note A.)

Of the methods of investigation founded on these principles, those which depend upon the exercise of vision and tact, have been employed from time immemorial. Of the three methods derived from the exercise of the sense of hearing,—Percussion, Succussion, Auscultation,—the two first only were rendered of any practical utility by the ancients; for the remark of Hippocrates referring to auscultation, cited by Laennec,¹ was an almost isolated observation, which produced no result, and did not even attract the attention of his commentators. (Note B.) The circumstances in which percussion and succussion were employed by the ancients, are so comparatively unimportant and rare, that we may in point of fact consider the appli-

¹ Vol. I, p. 37, 3d edition.

cation of the sense of hearing to the direct exploration of the internal organs, as an invention originating with Avenbrugger, in 1761; forced upon the public attention by Corvisart, in 1808; organized, extended, and perfected, by Laennec, in 1815. The treatise of Laennec on mediate auscultation, was translated by Dr. Forbes, in 1821; and thus the new methods of direct exploration can only be said to have been before the English and American public for the space of fifteen years. It is to be remembered, that in the very city where they originated, the discoveries of Laennec were not at once received without opposition. Thus in the first edition of the Dictionary of Medicine in twenty-one volumes, the author of the article Auscultation estimated so very moderately the importance of the new method of exploration, that after reading it, one might have been tempted to throw the stethoscope into the receptacle which holds the scale of Hippocrates and the balance of Sanctorius. It was from this article that Dr. Good derived one of his two authorities for undervaluing auscultation.¹ Unfortunately, the author and editors had committed so serious an error, that in the second edition of the Dictionary of Medicine, now publishing, it has been thought necessary to suppress it entirely, although its author is still one of the collaborators, and a popular professor in the School of Medicine; and the treatise which has taken its place is from the hands of one of the most ardent admirers of Laennec.

¹ Study of Med. iii, 207.

If the arts of direct exploration struggled with difficulties in Paris, it might well be supposed that they would fight their way with tenfold labor into general acceptance in medical communities where novelty excites and concentrates attention with less activity. This has been the case in our own country ; and at the present day it is well known that some practitioners of a certain merit, and particularly some who pride themselves on strong sense and intuitive sagacity, habitually neglect and depreciate the value of the physical signs, even of thoracic disease.

Several causes have combined to produce this indifference and disbelief. We need not mention the reasonable scepticism with which those who have often been deluded by novelties, regard almost every innovation. But, if with the most startling novelty, and with extravagant promises, an invention come forward which demands the laborious training of a sense hitherto uncultivated in its more delicate capacities, bearing in one hand an instrument which is to be the practitioner's inseparable companion, and in the other a treatise full of new terms and peculiar doctrines, is it to be supposed that the schools of Sydenham and Cullen would consent, without a struggle, to recommence their education and remodel their nosology ?

It was perfectly natural that they should look with suspicion upon this introduction of medical machinery among the old hard working operatives ; that they should for a while smile at its pretensions, and when its use began to creep in among them, that they should observe and signalize all the errors and defects which happened in its practical application.

And erroneous conclusions are formed by those who employ direct exploration, as well as others.

They are sometimes careless in their examinations, like other people who profess to study nature ; they draw positive conclusions from insufficient premises, as often as other observers. There is sometimes a tendency in those who have long employed the physical signs, to carry their observation to that infinitesimal minuteness which the tare and trett inseparable from the mass of evidence, renders entirely fallacious. But all this is true also of the chemist, whose balance turns on plates of polished crystal ; yet who would found an argument on the errors of careless analysts, or the fractional differences in the tables of Thenard and Berzelius ?

If mistakes are sometimes committed by those who have studied the art of direct exploration under the most favorable circumstances, with the best instructors, and amidst large numbers of patients, we may well suppose that such as have been obliged to learn only from books, and without the opportunity of attending ample hospitals, must be still more liable to errors. That such have been committed is probable ; and instances considered as such, we have all heard brought forward as proofs of the inadequacy of the methods under consideration. Some of the sources of error we have alluded to ; the principle in logic or out of logic, which turns them into an argument against the art, we have seen too often exemplified in attacks upon the whole science of Medicine, founded upon the errors and discrepancies of those who exercise it.

The question before the Medical profession, is not whether the arts of direct exploration are new ; it is

not whether they require much study ; it is not whether mistakes may be or have been committed ; for even if a long catalogue of them could be accumulated, it would prove no more against the method, than a history of the aneurisms which have been opened as abscesses, or of the solid tumors whose arteries have been tied as aneurismal, would prove against the utility of the surgeon's examining tumors.

The questions before the profession are : Are the new methods of exploration founded on rational principles ? Do they lead to any useful results ? Are they received as important by those whose opinion is most to be relied upon ?

It requires little to show that they are founded on correct principles. That a cavity with yielding walls must be dilated by the accumulation of fluids within it ; that the impulse of a heart tripled in volume and in vigor, must be more than usually sensible to the hand ; that a chest crowded with liquid must return a different sound when struck upon, from that of the healthy state, in which even Virgil knew enough of percussion to say,

—pectore vastos
Dant sonitus¹—;

that the movements of a fluid which only partly fills the pleura, may be perceptible to the patient and those about him, as well as those of liquid in the stomach or bowels ; that the rattling of different morbid secretions in the bronchiæ may be heard on applying the ear to the chest, when, in the trachea, which

¹ Æneid, V, 434.

is only a larger bronchia, they may often be heard through a whole apartment;—all these propositions are sufficiently in accordance with physical laws to obtain an easy assent.

Do they produce any useful results? They enable us, in many cases, to distinguish phthisis before its existence is shown by the general symptoms, at a period when we can say, do this and you may live, do that and you must die; they enable us to determine the stage and the degree of pneumonia, even when its rational signs are wanting, at the time when treatment is most effectual; they reveal to us the existence of pleurisies, which, clear as the day when properly sought after, have yet been long wasting the patient and perplexing the physician; they declare so distinctly the existence of pericarditis, that this once dreaded and most obscure disease may be recognised even by the half taught student; they have, in a word, rendered the derangements of the very organs which nature seems to have barred and bolted from our reach most carefully, the best understood of all those which affect the organs of the visceral cavities.

What has been the decision of those whose opinions are most valuable with regard to the arts of direct exploration?

We have seen how the opinion of the editors of a great public work, the Dictionary of Medicine, had changed in the interval of publication of its first and second editions. We can bring forward testimony which has different claims to attention. It is rare to find a less amiable feeling entertained by the living for the dead, than the father of the “physiological doctrine” has expressed in his numerous allusions to the

illustrious inventor of auscultation.¹ Even he, however, can use the following language. "I must render justice to the sagacity with which M. Laennec is able to discover and to follow in its progress the disorganization of the lungs by the employment of his cylinder. I make use of it every day with the highest advantage. Without this ingenious instrument, we could obtain only approximative notions of the existence of purulent collections, and of the different degrees of permeability of the pulmonary tissue. With it, all these questions are resolved in the most satisfactory manner."² M. Bouillaud, a representative of many of the prejudices as well as doctrines of Broussais, and certainly no idolizer of the "School of La Charité," as he calls his great predecessors and their adherents, makes use, however, of direct exploration with a zeal which the succeeding pages will testify. M. Piorry has succeeded in creating himself an European reputation, by a slight but useful modification in the single art of percussion. Not only are the classical works of Louis and Andral filled with the applications of the art of direct exploration, but the stethoscope and the pleximeter are to be found in the hands of every Parisian student, and what is still more, of gray-headed professors who were no longer young when Laennec published his discoveries, as means of investigation which it would be ignorance not to understand, and unpardonable carelessness to neglect. If we look at the English physicians, whom we are accustomed to believe less ardent for curious

¹ Vide Exam. des doct. Méd. Vol. IV, p. 163—366, etc.

² Op. cit. iv, 147.

innovations, the pages of the London Cyclopædia of Practical Medicine, filled by the most distinguished practitioners in the country, bear witness to the immense value of the new resources which have been offered to science. The names of Clark, and Forbes, and Hope, may fairly be weighed against that of the flippant scribbler of a page in the Athenæum, who tells us, in speaking of the stethoscope, that "the toy is a new toy" more than twenty years after its invention. If Harvey, among all his opponents, deigned only to answer Riolanus, and that on account of his rank, fame, and learning, the inventor of the *new toy*, were he still living, would have even fewer controversies than the discoverer of the circulation. It would be easy to multiply names, throughout scientific Europe and America; but we are unwilling to carry this invidious argument farther, especially as it is fully understood by all who keep pace with the current medical literature.

We shall proceed to examine the use and importance of direct exploration in the most important diseases to which it may be applied with a prospect of utility.

DISEASES OF THE LUNGS.

Tubercular Phthisis.

It is so well known that the organic lesion which tends to produce the array of symptoms, from which the name of this disease was derived, frequently exists without betraying itself by these characters, that it is unnecessary to support this point by particular

evidence. The insufficiency of the general symptoms, in many cases, to reveal its existence, and the importance which has been attached to its early discrimination, may be estimated by the numerous experiments which have been instituted in ancient and modern times, to afford the means of distinguishing purulent expectoration from that resembling it in character, and supposed to be less significant of danger. It is now well known that whatever be the degree of their accuracy, they are of little value, from the fact, that mucous membranes may secrete pus during inflammation; and that on the other hand, the existence of tubercular disease in the lungs is by no means always attended with purulent secretion.

If we can detect the existence of tubercles in their crude or primitive state, we discover the disease nearer its origin, than the experimenters we have referred to even attempted; for it is proved by dissections, that these bodies, and even excavations in the lungs, may precede the existence of either cough or expectoration;¹ and they are found in the bodies of patients who have died of various diseases, and in whom no thoracic symptoms have excited attention.²

Can we detect their existence in their different stages by means of the physical signs? Let us examine these signs in succession.

The value of *inspection* of the thorax, it is not easy to determine; for means so much more exact are commonly employed, that we are too apt to overlook

¹ Louis, Rech. sur la Phthisie, Cases 31, 32. Andral, Clin. Méd. II, p. 112-13.

² Laennec, I, 63, (Paris, 1831.)

the assistance it may afford us. A phenomenon evident to inspection, namely, the immobility during respiration of those parts of the chest which correspond to a collection of tubercles, is mentioned by Andral,¹ and again by Dr. Clark,² as affording valuable information. Laennec says, on the contrary, "I have never been able to find manifest and constant inequality in the movements of the two sides of the thorax, except in cases of very abundant empyema, or of deformity of the thorax."³ M. Louis does not advert to this sign in speaking of the diagnosis of phthisis, and although in the habit of inspecting the chest with great minuteness, he does not habitually call the attention of his students to this circumstance. It has been said also, that the depression of the thoracic parietes follows the cicatrization of tubercular cavities;⁴ a circumstance probable enough in itself, but which I do not remember to have witnessed.

The utility of *palpation*, in the diagnosis of phthisis, is very limited, from the nature of the thoracic parietes. It is only capable of transmitting to our perception certain movements which take place in the lungs, and which may be nearly as well appreciated by the ear, which in this case becomes an organ of tact as well as of hearing. In applying the hand to the chest of a tuberculous patient, we may sometimes feel the movement of fluids in the bronchiæ or tuberculous cavities, and on making him speak, a thrill of unnatural force may sometimes be felt over the seat of tubercles or excavations.

¹ Clin. Med. II. 97.

² Lond. Cyc. Art. Tubercular Phthisis.

³ Vol. I, p. 32.

⁴ Laennec, II, 341. Andral, II, 97.

I remember the case of a woman, in whom percussion and auscultation gave indubitable evidence of the existence of extensive tubercular disease at the upper part of the right lung—disease of which she died in less than two months from the time when I had first seen her. This patient complained to me of a jarring or thrilling sensation beneath the right clavicle whenever she was speaking, and so much had it excited her attention, that she had a constant disposition to place the left hand upon this region; where, upon placing my own, the vibrations to which she referred were exceedingly evident.

We come to a more important means of examination in the disease we are considering; the use of *percussion*.

In those cases of tuberculous disease in which a large part of the lungs is changed from the condition of a light, porous, and consequently resonant body, to a dense and solid mass, no one can doubt that the sound returned on percussion must be modified. In point of fact, the difference is sometimes so great that the patient himself is alarmed by it. This is still more true when, with the solidification of the lung, there exist also caverns of sufficient extent to give rise to the sound which has been named from its resemblance to that obtained by striking a cracked vessel. It is often necessary in cases of this kind, to use percussion with very moderate force, in order not to render too palpable a sound so calculated to excite the fears of the patient and those around him. In proportion as the tubercular disease is less developed, the signs which are obvious on the slightest examination in the stage of advanced disease, and detected without much

difficulty at a period previous to that of extensive disorganization, require more care to elicit and to perceive. The testimony of Laennec,¹ of Andral,² and of Dr. Clark,³ agrees in the fact that a certain number of tubercles, if surrounded by healthy pulmonary tissue, may exist without giving evidence upon percussion. How great must be the change of structure in the lung before we can recognise it? No precise answer can be returned to this question; for it must depend in part upon the more or less superficial situation, upon the relative distance or proximity of the indurated masses with regard to each other, and upon the delicacy of the observer's ear, and his art in percussing. It is enough to say, that a degree of disease far short of that commonly found in declared phthisis, may be recognised by any person with senses of common acuteness when percussion is properly applied by another, and that the art of practising it with sufficient accuracy for many purposes may be acquired in a few trials. The author of a work on auscultation, M. Raciborski, when resident physician at La Charité, was in the habit of delivering a course of complete clinical instructions in auscultation and percussion, in about a dozen lessons. However insufficient this time may have been for the purpose, yet it may give some notion of the facility with which the ear appreciates the more striking phenomena; and among them none is more readily seized than a difference of sonorousness in the two sides of the thorax.

An instance of the accuracy of the evidence afforded

¹ Vol. II, 127.

² Clin. Méd. II, 260.

³ Lond. Cyc. Art. Tub. Phthisis.

by percussion, may be found in the twenty-ninth case reported in the Memoir of Dr. Jackson,¹ in which the resonance was declared obscure at the right summit nearly four months before death, and yet on examination after death, the disease was still found in its first period.

It has been said² that the results of percussion may be rendered deceptive by the co-existence of emphysema, or dilatation of the vesicles, with tubercles. In the series of emphysematous patients examined after death by M. Louis, tubercles were a little less common than among the common average of subjects who had died of various diseases. Still, in a considerable number of cases, the two affections were found in different degrees to co-exist; in four out of forty-two cases, the emphysema being strongly marked, and in six others slightly so, while the tubercles in all but one instance were few in number.³ We may concede, then, that in certain cases percussion, if employed alone, would leave doubts respecting the diagnosis, and that we must have recourse to the other methods to decide the question. These will generally be sufficient. M. Louis himself, in speaking of a patient under his care, did not hesitate to announce as his diagnosis, after repeated examinations, the existence of tubercles *in an early stage*, and emphysema *strongly characterized*, upon the right side of the chest; ⁴ it must be still easier to detect a few tuber-

¹ Memoir of James Jackson, Jr., M. D.

² Andral, II, 61. Clark, Lond. Cyc.

³ Louis, Rech. sur L'Emphysème des Poumons, p. 180. Mem. de la Soc. Méd. d'Observation. Paris, 1837.

⁴ Ibid. p. 245.

cles in conjunction with slight emphysema, and easiest of all to discover a large collection of tubercles in connection with the same trivial lesion. We may remark here that the invention of mediate percussion, which Laennec did not employ, and which was brought before the public about the time that Andral published his *Clinique Médicale*, has rendered our appreciation of the resonance much more precise, and undoubtedly given us positive signs in some cases where the method of Avenbrugger would have been insufficient.

Like the other signs of tubercular disease, those derived from percussion should be sought for especially about the clavicular and acromial regions. Obscurity of resonance being detected beneath one or both clavicles, or at any part of the chest corresponding to the summit of the lungs, what conclusion is to be formed with regard to the nature of the disease? I answer this question by a remark of Chomel in one of his lectures.¹ "Obscurity of sound and feeble respiration under one of the clavicles, give strong reason to suppose the existence of tubercles; for partial effusions take place, in the immense majority of cases, at the inferior and posterior parts of the chest, while chronic pneumonia, primitive, and without the presence of tubercles, is scarcely known."

The information afforded by *auscultation*, relates either to the character of the respiration, to its complication with certain rattles, or to the resonance afforded by the cough or voice.

It is necessary to concede with regard to this method

¹ MS. Notes.

of exploration, as we have done for percussion, that there may exist a limited number of tubercles in the lungs which shall elude its researches.¹ Let us remark, that the conclusions of Laennec, of Andral, and of Louis, as recorded in the works we have quoted, are not to be taken as the standard of our present knowledge of the subject. Thus, the first of these authors passes rather lightly over the changes in the character of the respiration which reveal the existence of tubercles.² M. Andral remarks, that the respiration may be natural, or in excess, or deficient, that it may be accompanied by different rattles, or bronchial in its character.³ M. Louis, in speaking of this first period of tubercular disease, tells us that the respiratory sound was either unchanged in character, or weakened, or attended with mucous or sonorous rattles.⁴ More recent observation has detected certain changes in the character of the respiration during the early stages, which afford additional information. It is very common to hear M. Louis declare the respiration "rude" or rough under one of the clavicles. This he considers as affording strong evidence of tubercular disease. One of our American students, whose loss Science will long lament, has carried the accuracy of observation still farther, by examining separately the sounds of inspiration and expiration.⁵ In the early stages of tubercular disease, he observed the existence of a prolonged expiration, having something of the bronchial character. The cases referred

¹ Louis, *Phthisie*, 182. Andral, II, 68.

² Vol. II, p. 127. ³ *Clin. Méd.* Vol. II, pp. 69, 76. ⁴ *Phthisie*, 182.

⁵ *Memoir of James Jackson, Jr., M. D.*, pp. 341, et seq.

to by him render it probable that this sign will take its place as one of the earliest indications of the disease we are considering; and I may add that it is frequently referred to by M. Louis as affording probabilities of its existence. But notwithstanding the value we attach to this sign, it is not to be considered, when alone, as a proof of the existence of tubercles. I examined a woman three years ago, in company with a gentleman very accurate in auscultation, in whom both of us recognised a distinctly prolonged expiration at the posterior and upper part of the right lung. Within a day or two this patient was seized with cholera, and died within twenty-four hours from the attack. The upper lobe of the right lung was examined with scrupulous care, and offered nothing in its substance or bronchiæ to account for the circumstance we had observed. This case goes to confirm an opinion expressed by Dr. Gerhard.¹ "The sound in the right lung of a phthisical patient should not be regarded as morbid, unless the respiration is decidedly blowing; a slight difference perceptible by an accurate auscultator, does not necessarily indicate disease." He adds, "But if the respiration be more blowing at the summit of the left than the right side, there can be no reasonable doubt that the lung contains tuberculous matter." This circumstance he attributes to the different arrangement of the bronchiæ on the two sides.

The existence of distinctly marked bronchial respiration at one or both summits is a sign of great im-

¹ On the Diagnosis of Diseases of the Chest, p. 166, (Philadelphia, 1836.)

portance. It is true, as Andral says, that "it announces only the impermeability of the pulmonary tissue."¹ This sign may also exist in pneumonic hepatization of the upper lobes; but the date and symptoms of the disease can rarely leave it doubtful to which of these two affections it is due in any given case, and the other lesions which may occasion it are so comparatively rare, that this single phenomenon is sufficient to give very strong presumptions of the existence of tubercles, whenever the affection is of a chronic nature. We can only consider the remark of Andral, that "it cannot serve to discover the existence of tubercles,"² as a careless expression, which means only that it does not by itself constitute an absolute proof of their existence.

The presence of cavernous or amphoric respiration at the summit of the lungs, is satisfactory proof of the existence of a cavity, which ninety-nine times in a hundred is the consequence of tubercular disease, and in very rare cases, of dilatation of the bronchiæ; an instance of which may be found in the eleventh case of Louis.³

The rattles which are found in the early stage of phthisis, may be of somewhat various character, sometimes being but a rare or solitary crackling, and at others a more or less large and regular subcrepitous or mucous bubbling. At a later stage, when cavities have formed in the lungs, a distinct gurgling may often be heard over their situation. But whatever be the character of the rattle, the most important circumstance is its position. Bronchitis, as is

¹ Clin. Méd. II, 76.

² Loc. cit.

³ Phthisie, p. 235.

frequently observed by M. Louis in his lectures, almost universally affects the lower and posterior parts of the lungs, and Chomel remarks, in speaking of emphysema, that it is almost always more marked near their base than their summit. The law which determines that tubercles shall commence at the upper part of the lungs is so general, on the other hand, that of one hundred and twenty-three cases analyzed by M. Louis, in all but two they were more numerous, larger, and more advanced in their development at the summit, than at the base.¹

The presence of the gurgling sound at the superior parts of the lungs, gives strong presumptions of the existence of a cavity. But even this, as Andral remarks, cannot be considered as a true pathognomic sign of the existence of phthisis, for he has found a true gurgling sound in cases of simple bronchitis.² This is particularly liable to occur over the points occupied by the large bronchiæ. The induration of the lung appears to give greater intensity to this sound—for I have heard it in one case at some distance from the chest, in a patient whose lungs were condensed by tubercular matter, and in which no excavation or dilatation of the bronchiæ was noticed after death. As this, however, is a sign which is shown every day to coincide with tubercular cavities, these exceptions, while they take from it the character of mathematical certainty, do not destroy its claim to the rank of a valuable element in diagnosis, especially when it is found in that part of the chest corresponding to the usual region of tubercular disease.

¹ Phthisie, p. 2.

² Clin. Méd. II, 79.

There remain the phenomena afforded by the *voice* and *cough*, as explored by auscultation. I cannot but think that the minute distinctions of Laennec, between bronchophony and perfect and imperfect pectoriloquy, have constituted one of the most perplexing and repulsive points to the learners of the art of auscultation. To speak of the tones of the voice being heard a *short distance up the stethoscope*, for instance,¹ is to present to the student a distinction of such tenuity as must seem beyond the reach of his faculties. That his doctrines have led into error I am confident. The fallacy of pectoriloquy as a positive sign of the existence of caverns, is shown by its occasional presence in consequence of hepatization. As this is a point of some consequence, I shall adduce authorities in support of this assertion. "It must not be forgotten that without the existence of any tubercular excavation, and by the sole fact of considerable induration of the pulmonary tissue, the voice may present a resonance more or less resembling that of perfect pectoriloquy; under which circumstances, according to Laennec, it constitutes bronchophony; but if it is true that these phenomena are only separated by slight gradations, we must perceive how they will tend to run together, so that they can only be distinguished by an infinitely practised ear."²

"Pectoriloquy may exist in different degrees of tubercular disease, pulmonary induration and effusion."³

"I may be allowed to mention here, that I have upon an important occasion (Concours, etc.), esta-

¹ Vol. II, p. 182.

² Clin. Méd. II, p. 82.

³ Chomel. MS. Notes.

blished in a positive manner, that pectoriloquy, considered previously as the exclusive character of cavities in the lungs, exists in pneumonia with induration."¹ In the article Auscultation, in the Dictionnaire de Médecine ou Repertoire, etc. it is said by Dance, that pectoriloquy "may be confounded with bronchophony, and reciprocally, if we trust only to the modification of resonance of the voice."

It seems as if the inventor of auscultation, in his anxiety to uphold the constant coincidence between pectoriloquy and pulmonary excavations, had somewhat forgotten the principles which he commonly followed with such rigor. He uses the following expressions:—"Pectoriloquy is doubtful when the resonance is very feeble, and cannot be distinguished from bronchophony except by the aid of signs deduced from the place where it exists, the general symptoms and the progress of the disease."² It is very clear that these circumstances may help us in the diagnosis of the disease, but they cannot make a difference between two sounds which he has already declared undistinguishable in themselves.

At the same time that we point out certain restrictions, which it is necessary to remember, if we would not sometimes fall into error, the unnatural resonance of the voice is to be ranked as one of the most constant, the earliest, and most delicate signs of the presence of tubercles. It is not uncommon to find it, when the rational symptoms are such as to excite little attention, attended perhaps by a slight

¹ Cruveilhier, article Pleurisy. Dict. de Méd. et de Chir. pratiques.

² Laennec, I, 67.

change in the respiration, but without the presence of rattles, and accompanied by very tolerable resonance of the chest. Even the perfect transmission of the voice is sometimes found in such cases. "More than once," says Dr. Williams,¹ "has it occurred to me that the very words, which in that delusive confidence with which this malady enshrouds its victims, ridiculed my examination of the chest, roundly saying, that nothing ailed them there, have belied their meaning, and coming from the breast, have told a far different tale!"

A phenomenon which has never to my knowledge been pointed out, consists in a confused jarring, distinguishable while the patient is engaged in conversation, and which is probably a true bronchophony heard at a distance from the chest. This character of voice perhaps corresponds to the "*vox clangosa*" of Morton.² I have found, since observing this phenomenon, that M. Piorry has detected a similar fact with regard to hægophony. "In almost all the old women observed at Salpêtrière who presented hægophony when examined by auscultation, the voice listened to directly, as it came from the mouth, offered the same character."³ (Note C.)

The signs revealed by the cough are sometimes useful as auxiliary to those afforded by the respiration and voice, but do not require any particular examination independently of the others.

The increased sound of the heart over the points

¹ Rational Exposition, p. 184, note.

² Phthisiologia, Lib. II, Cap. 3.

³ Procédé opératoire, etc. p. 61.

invaded by tubercles, which has been noticed by Dr. Gerhard¹ as a new sign, and attributed to M. Louis, does not appear to merit that appellation. Although Laennec, in treating of the physical signs of tubercles, does not mention this circumstance, yet when speaking of the extent of the pulsations of the heart,² he uses such expressions as the following—"thus, if there are tuberculous excavations at the summit of the right lung, the pulsations of the heart will be heard more distinctly under the right clavicle and axilla than under the left, and sometimes even than in the cardiac region." A little above, he speaks of this circumstance as difficult to explain, "unless we suppose that the sound is transmitted in this case, not through the excavations, but through the medium of their engorged and condensed parietes." But this sign certainly assumes a new value, if, as Dr. Gerhard asserts, and as we believe correct, it is sometimes present at a very early period of the disease.

We offer the following propositions as forming our conclusions upon the collective value of the physical signs of tubercles.

1. There are cases in which tubercles exist in limited number without giving rise to any appreciable change in the results of direct exploration.

2. None of the signs of tubercular disease can be considered in itself as absolutely pathognomonic.

3. The co-existence of several signs, their being found at the points disposed to tubercular disease, the great frequency of tubercular disease, the extreme comparative rarity of any other lesions capable of pro-

¹ Op. cit. pp. 108-109.

² Vol. III, p. 8.

ducing the same physical signs, enable us in many cases to arrive at a degree of probability which is for practical purposes equivalent to certainty.

4. The co-existence of physical signs not in themselves absolutely decisive, with general symptoms insufficient alone to characterize the presence of phthisis, may authorize us to consider the patient as affected with that disease.

The application of the knowledge derived from direct exploration may be made to three classes of patients.

First, those in whom the general symptoms have excited no suspicions of phthisis. These latent cases have not been unnoticed heretofore by accurate observers. Thus we have been warned, in the course of our studies, to suspect *emaciation* as a sign of the existence of tubercles, even without any local symptom. It is probable that an auscultator of the present day would form a very different opinion of those cases which induced Dr. Percival to say, "I willingly subscribe to idiopathic hectic, and have known it to last three months without any pulmonary affection, and then to break out in the lungs."¹ On referring to my notes upon this subject, I find the following remark of Louis. "In certain very rare cases, patients die under hectic fever, with tubercles which had been recognised by auscultation, but having never been affected with cough."

By cases disguised in a different manner, it is probable that Wilson Philip was misled in the creation of his celebrated entity, Dyspeptic Phthisis. There can

¹ Good, Study of Med. Vol. II, p. 152.

be little question for those familiar with direct exploration, that in cases in which, according to that author, "the patient lives until almost the whole lungs are rendered incapable of their function,"¹ a few moments devoted to percussion and auscultation would have detected the principal disease, while he was speculating about the connection of the liver and the spleen, and losing himself in the divisions of the cœliac artery.

The practical importance of ascertaining the existence of tubercles in an unsuspected patient, is obvious. We no longer treat the insignificant or deceptive symptoms which may have shown themselves, as unimportant, or with reference to some imaginary disease. The discovery of their existence authorizes us to recommend the change of habits and of climate in certain instances, where, but for these means of exploration, we might have dallied with the disease until there was no longer any hope of suspending its progress. It is beyond doubt that external circumstances possess a considerable influence over the course of tubercular disease; this has long been known with regard to patients affected with hæmoptysis and the general symptoms of tubercles; facts like that in the twenty-seventh observation of Laennec, which are not unfrequent, prove that the existence of structural changes, as shown by direct exploration, does not preclude the hope of amendment under favorable influences. Those who have studied the disease attentively, among that class of patients who possess the means of changing their climate and

¹ On Indigestion, p. 166.

manner of life, would probably be able to offer many examples of this nature. Let those who are fond of telling us we have gained nothing by the attention paid of late years to the history and the signs of phthisis, reflect upon a remark in Dr. Clark's admirable essay on this affection. "The beneficial effects of a residence for one or two winters in Maderia, have become much more apparent since the public have been impressed with the necessity of adopting change of climate rather as a preventive than as a means of cure. A few years ago it was a matter of little moment to select a climate for the consumptive patient, because he was generally in the advanced stage of the disease, without hope of recovery, before the measure was proposed or adopted; and its fatal termination was not unfrequently accelerated by the only means to which he looked for safety."¹

This is not the place to expose the different methods of treatment which may be employed with hopes of utility, under the circumstances we are supposing; the key to them all lies in the discovery of the fact, that the patient is laboring under a disease whose future tendencies we know but too well when its presence has been determined.

The second class of patients comprehends those in whom the existence of phthisical symptoms has excited suspicions of this disease, which are converted into certainty by the results of direct exploration. It is for this class of patients that the use of this

¹ London Cyclopædia.

method of forming a diagnosis is most frequently employed. The presence of the physical signs of tubercles, in such cases, presents us with definite indications, the application of which is to be modified by the degree to which the constitution is undermined by the general symptoms. Other things being equal, the extent to which the disorganization of the lungs has proceeded must influence the prospects of treatment, and particularly must enable us to settle the delicate question which arises so frequently, whether the patient shall remain at home, or in the prospect of suspending his disease, undergo the fatigues and trials inseparable from change of climate, and expose himself to the chance of dying among strangers.

In the third class of patients, those upon whom the seal of tubercular disease is impressed so deeply, that, as in the days of Aretæus, it is obvious to the first passing stranger who looks upon the wasted victim, it might be thought that direct exploration could be of no utility. Certainly in these circumstances we should deprecate the scientific zeal that would harass a suffering and exhausted being for the sake of gauging with idle minuteness the extent of the organic changes which are destroying him. It is too true that we have sometimes been reminded of the line of the satirist

'Centum me tetigere manus aquilone gelatæ'

in clinical examinations. But if such abuses were matter of sarcasm in the time of the Romans, they are not to be all laid to the charge of the modern methods of examination, which, when executed with

delicacy and attention to the patient's comfort, and not too long continued or too often repeated, are seldom the source of uneasiness. And it is not to be imagined that even at this period they are altogether fruitless. They at least, as Louis remarks in his lectures, give us some light upon the probable duration of the disease. For of two patients evidently phthisical, whose disease is of the same standing, the one in whom the disease has produced the most extensive disorganization, must anticipate its more rapid progress and more speedy termination.

The utility of direct exploration in the most important complications of phthisis, will be considered in connection with pneumonia and pleurisy.

We have conceded that a certain number of tubercles might sometimes exist without rendering themselves evident by distinct physical signs. In such cases we are obliged to rely upon the general symptoms, which may in themselves be sufficient to authorize us to consider the patient as tuberculous, and treat the disease upon that supposition. The remarks of Dr. Gerhard upon this subject,¹ and the case which he has reported in illustration of them, deserve the profound attention of every student of the arts of direct exploration.

With all the certainty which these methods have given us, we should not be induced to overlook the importance of those functional disturbances which, it may be, frequently precede the organic changes which we detect at a later period. The student who reads the treatise on auscultation of Laennec, should know

¹ *Diagnosis of the Diseases of the Chest*, pp. 11, 12.

by heart the chapter of Morton, on the precursory symptoms of phthisis.¹ By the proper employment of the rational and physical signs, which illustrate the obscurities and supply the deficiencies of each other, we may often detect the presence of tubercular disease before that melancholy period has arrived, “ubi incassum ab arte Medicâ expectantur miracula, cum de animæ futurâ salute, et testamentis faciendis Theologum, et Jurisperitum consulere magis conveniat.”

Pneumonia.

The use of direct exploration may enable us to determine the existence of this affection, when the general or local symptoms are insufficient; or it may teach us its stage, situation, extent and progress, the nature of the affection being ascertained.

Sufficient evidence of the presence of pneumonia is not always afforded by the rational symptoms. Cough, pain in the chest, febrile movement, may be owing to pleurisy, or to pericarditis, or to bronchitis. The characteristic sputa may be wanting; in the words of Andral, “at other times pneumonia passes through its different stages without having been in any way announced by the expectoration, which has been wanting, or without character.”² In his thirty-eighth case, the expectoration was suppressed on the eighth day, and did not return during the rest of the disease. In his forty-second and forty-third observations, where the existence of pneumonia was proved

¹ Phthisiologia, Lib. II, Cap. 2.

² Clin. Méd. I, p. 531.

by the examination after death, the expectoration was either absent or catarrhal; yet one of the patients was seen as early as the morning of the third day.

In such instances, if there are any positive signs to denote the presence of the latent disease, they must be useful and important. Let us examine the phenomena afforded by direct exploration, in the different *stages* of the disease.

A slight degree of flatness on percussion is now recognised as a sign of the first stage of pneumonia,¹ (engorgement—splenization).

It is probable that the manner in which percussion is now performed, by means of the pleximeter, has enabled us to recognise this slight degree of flatness, which seems to have been overlooked by Andral, who remarks, "In cases of pneumonia, the sound never grows dull until about the second or third day, sometimes later."² He must have referred to the flatness produced by hepatization, and consequently have overlooked the less obvious diminution of resonance which precedes this condition. At this same stage of the disease the crepitant rattle makes its appearance.

Laennec asserts that this sign always exists, and from the first moments of the disease.³ He remarks that it existed in all the cases reported by Andral, in the *Clinique Médicale*, (sixty-five in number), with the exception of seven; and he adds, that at that time M. Andral was little accustomed to auscultation. Of eight cases reported in the *Memoir* of

¹ Bouillaud, *Dict. de Méd. et Chir. prat. Art. Pneumonie*. Williams, *Lond. Cyc.*

² *Clin. Méd.* I, p. 530.

³ *Auscult. Méd.* I, p. 417.

Dr. Jackson, five of which were fatal ones, every one offered this sign. The presence of the crepitous rattle, notwithstanding it has been also attributed by Laennec to two other affections, pulmonary apoplexy and œdema, is one of the most decisive signs of pneumonia. For, with regard to pulmonary œdema, it is rarely idiopathic, commonly accompanied with other forms of dropsy, in cachectic subjects, or the sequela of pneumonia, catarrhs, or measles.¹ The rattle itself is rather sub-crepitant than crepitant; the bubbles are larger and moister than in pneumonia.² The other affection in which Laennec found the crepitous rattle, has not presented it to some observers, as in a case which occurred to M. Cruveilhier, and three cases seen by M. Piorry.³ We must rely on the local and general symptoms to distinguish the two affections, particularly the existence of hemorrhage, and the absence of strong febrile action in the case of pulmonary apoplexy. In a disease which has recently attacked a person previously well, and is accompanied with cough and violent febrile movement, the existence of the two signs which have been mentioned, slight flatness on percussion, and the crepitous rattle, can leave little doubt that the substance of the lungs is inflamed. There is no question that mistakes have been committed not unfrequently with regard to the characters and the presence of the latter sign; and I recall one in which such was probably the case, from the observer's not being sufficiently familiar with this peculiar rattle.

¹ Auscult. Méd. Vol. I, p. 336.

² Ibid.

³ Ibid. Vol. I, p. 373, note.

If the examiner is well acquainted with this rattle by having studied it upon well marked cases of pneumonia; and those which resemble it, in distinctly characterized instances of bronchitis, he will find few instances in which his ear will not at once decide on the nature of the disease from this single phenomenon. We are assisted in forming our opinion, by observing whether a doubtful rattle is heard on one or both sides of the chest; for, as Louis remarks, pneumonia is commonly confined to one side, while catarrh ordinarily exists on both.¹

The second stage of pneumonia, that of hepatization, is distinguished by signs proportionate to the singular change of structure undergone by the pulmonary tissue. The sound on percussion is entirely flat—"tanquam percussi femoris."

The natural murmur is suppressed and gives place to the bronchial or tubular respiration; the voice is heard with unnatural force over the parts affected, sometimes with a startling proximity to the listener's ear. We have previously mentioned that the character of the increased vocal resonance may be that, which, when heard over a cavity in the lungs, is called pectoriloquy. If, as is common in this disease, there is also a slight pleuritic effusion, the voice may have something of the bleating character which is found in pleurisy; a modification designated by M. Bouillaud as broncho-ægophony.² So constantly do the physical signs of hepatization exist in pneumonia, that I have heard M. Louis say in his lectures, he had not seen a case during five years in which the

¹ Lectures.

² Dict. de Méd. et Chir. prat. Article Pneumonie.

affection went on to recovery without this change having taken place. At this epoch it is difficult to confound pneumonia with any other affection excepting pleurisy. The entire absence of respiration at the points occupied by the effusion, the presence of marked hægophony, the dilatation of the chest in the latter; the crepitous rattle at the edges of the parts flat upon percussion, with bronchial respiration and bronchophony over the parts without resonance, in the former; such are the signs which will generally enable us to draw the distinction. Mildness of the general symptoms is strong reason in a doubtful case to suppose the existence of pleurisy.¹

The third stage of pneumonia, purulent infiltration, has little in its physical signs to distinguish it from the second, unless it be the rattle in the bronchiæ, owing to their more copious secretions, or to the pus poured into them at this epoch of the disease.² We are obliged to form our opinion principally from the date of the disease and the character of the expectoration.³ With regard to abscesses after pneumonia, as they are so rare as to form pathological curiosities, we need hardly fear mistaking them for tubercular cavities, with which their physical signs must substantially agree.

- The resolution of the disease is announced by the reappearance of the crepitant rattle at the points from which it had disappeared during hepatization, and the return of the vesicular murmur, with the gradual diminution of the flatness on percussion, the bronchial

¹ Chomel, Lectures.

² Laennec, I, p. 422.

³ Andral, Clin. Méd. I, p. 538.

respiration and bronchophony. The two last phenomena sometimes persist a long time after the activity of the disease has subsided. I have indeed seen it long after convalescence was fully established. In these instances, there is probably an engorged or œdematous state of the lungs. At this period M. Louis is in the habit of applying a plaster of Burgundy pitch, which has seemed to him to hasten the resolution of this morbid state of the pulmonary tissue.

It is clear that in ascertaining the presence of the physical signs of pneumonia, we also learn its *situation*. It is a very important point to decide whether it occupy the upper or the lower lobes. If it occupy the former, as was first remarked by Andral,¹ and has been confirmed by Louis and Chomel,² the prognosis is less favorable.

This circumstance is attributed by M. Louis to the age of the subjects, but it seems probable enough that the situation of the disease in itself should influence its issue. Some remarks of M. Bouillaud favor this supposition.³ In the case of pneumonia complicating a tuberculous affection, it appears from my own observation, and the cases given in the Memoir of Dr. Jackson,⁴ that the intercurrent affection tends to attack the superior and anterior parts of the lungs. In one instance I have seen a tuberculous patient struck down at once by intense pneumonia occupying the summits of both lungs, and carried off almost with the rapidity of cholera. At the anterior and superior part of the chest on both sides was an explosion of

¹ Clin. Méd. I, p. 569.

² Lectures.

³ Dict. de Méd. et de Chir. Art. Pneumonie.

⁴ P. 329, etc.

crepitous rattle at every inspiration. Although such may be the gravity of *double pneumonia* complicating tubercles, still, according to M. Louis, under these circumstances, the patient often recovers, whereas it is generally fatal in patients previously healthy. These remarks illustrate the importance of attending to the previous state of health of patients attacked with acute diseases, by which precaution we may often arrive at the knowledge of antecedent disease, which materially affects their prospects of recovery. A circumstance which renders the utility of direct exploration more general, is that pneumonia rarely remains profound, but arrives at the surface, where the ear can detect it.¹

The limits to which the physical signs are confined must obviously be those of the disease. These are often pretty exact ; thus, it is common to find the flatness on percussion, bounded anteriorly by a vertical line passing through the axilla. Other things being equal, the gravity of the disease must depend upon its *extent*, and therefore our knowledge of this extent must form one of the data for the treatment.

At the same time that we learn the situation and extent of the disease, we acquire the means of determining its *progress* by a subsequent examination. We may follow it with an accuracy which shall approach that with which we mark the progress of erysipelas. As the limits of the disease are extended or narrowed in the interval of two examinations, we shall estimate its severity, its tendencies, the effect of

¹ Chomel, lectures.

treatment. If the points first attacked undergo resolution, this is considered favorable by Chomel, even although other points should be seized consecutively.¹

If such be the utility of direct exploration in pneumonia, even when affecting adults, its importance is more absolute in the same disease affecting children.

In a memoir upon this affection by M. Rufz,² we find ample testimony to this effect. "We have never seen young children expectorate. The valuable diagnostic sign in pneumonia, furnished by the sputa, is entirely wanting at this age." "As to the physical signs furnished by auscultation and percussion, it is upon them that the diagnosis of the disease principally rests, and we conceive that before their employment, the study of the pneumonia of young children during life must have been very obscure."

The signs obtained by auscultation and percussion were very similar to those found in adults, in children above the age of six, but in those of early age they offered certain peculiarities which may be briefly mentioned.

As in them the pneumonia was commonly double, the sound on percussion was obscure on both sides; so that it was necessary to compare, not the sonorousness of one side with that of the other, but that of both with the healthy standard.

Instead of the crepitant rattle there was commonly a sub-crepitant rattle, with large bubbles.

¹ Lectures.

² *Journal des Connaissances Méd.-Chirurg.* Sept. 1835.

The bronchial respiration was rough, short, blowing, without vesicular murmur.

There was no bronchophony, properly speaking, but M. Ruz has observed, while the child was crying, a sound like that of simmering water, or like the murmur of a shell.

These results are similar to those given by Dr. Gerhard, in the work we have already cited,¹ and were derived by these two observers from the laborious studies which they prosecuted together at the hospital for children in Paris.

We have seen that pneumonia may be but imperfectly characterized by the general signs in adults, and that its symptoms are peculiarly obscure in children. We have seen that percussion and auscultation reveal its existence by signs often unequivocal in themselves, and still more decisive when compared with the general symptoms. That the knowledge of the existence of this disease leads to important therapeutic indications will be generally allowed. The utility of blood-letting, for instance, is peculiarly insisted upon in this affection. "From the time of Hippocrates to the present day, pneumonitis has been considered as one of the disorders in which the abstraction of blood is productive of the most unequivocal good effects."² "Some theorists, heretics in medicine, have alone dared to proscribe this remedy."³ The use of tartarized antimony in this disease, has received the sanction of Laennec and Louis. If such

¹ Pp. 89, 90.

² Good, *Study of Med.* Vol. II, p. 330. Cooper's additions.

³ Laennec, I, 481.

and similar methods of treatment are called for in pneumonia, the signs which enable us to distinguish it are useful and important in medical practice.

We have seen that it could be distinguished *early*, and this renders its signs doubly valuable. Thus, Cullen tells us, "blood-letting will be more effectual when practised in the course of the first three days than afterwards."¹ Andral directs two or three bleedings in the first twenty-four hours, if the inflammation is not arrested.² In the same connection he speaks of cases undergoing resolution by the use of a bleeding of sixteen or twenty ounces at the commencement. Similar testimony is borne by M.M Broussais and Chomel.³ That even moderate bleeding is more useful when practised within the first four days than afterwards, is established by the tables of M. Louis.⁴ M. Bouillaud, who carries bleeding to a far greater extent (mean term 4 lb. 9 or 10 oz.; maximum 10 lb.), ventures to conclude, "by means of blood-lettings thus repeated one after another, to the extent of four in the course of twenty-four hours, we shall scarcely lose a single patient with pneumonia, in whom the disease is recent, of small or at least moderate extent, and has not yet reached the third stage"⁵ (purulent infiltration). I introduce this statement among the rest, without meaning to stand sponsor for its exactness, hoping for the sake of humanity that there may be more in the assertions

¹ First lines, etc. p. 363.

² Clin. Méd. II, 572.

³ Dict. de Méd. et Chir. prat. Art. Pneumonie.

⁴ Recherches sur la saignée, 2d edit. pp. 12-36.

⁵ Dict. etc. Art. Pneumonie.

of the enthusiastic professor of La Charité, than some have been willing to allow. It will be observed that the state of hepatization does not, according to him, prevent this treatment from being efficacious. This might have been inferred from the rapidity with which the change in question takes place; sometimes in twenty-four hours of disease, as in a case I have witnessed. We need only add that whatever local remedies are employed, whether depletive or revulsive, the surest guide for their application is a knowledge of the situation and extent of the disease.

Pulmonary Œdema.

We have already had occasion to refer to this disease in speaking of the diagnosis of pneumonia. According to Laennec and to M. Bouillaud,¹ it rarely constitutes an idiopathic and primitive disease. The same authors consider it as often succeeding to pneumonia. Laennec remarks, that although it commonly supervenes at the close of acute or chronic diseases, and often a few hours only before death, he has seen it in many cases last weeks or months, and in some cases seem to be idiopathic.² He declares that its symptoms are extremely equivocal; and the author of the article Œdema, in the Dictionary just cited, confirms, or more probably repeats his assertion. This writer tells us that by means of attentive exploration, we may not unfrequently recognise the existence of passive œdema. One of its marks is,

¹ Dict. de Méd. et de Chir. prat. Art. Œdème.

² Auscult. Méd. 1, 337.

diminution of sonorousness of the chest on percussion. But Laennec attaches little value to this sign, because the two sides are commonly both affected, and even if only one is so, the result of this method of investigation is unsatisfactory, doubtless because there is still much air in the vesicles.¹ The respiration is feeble, and a rattle is heard which Laennec calls "crepitant, or rather *subcrepitant*," the bubbles of which are "larger, and convey to the ear a more evident sensation of moisture."² I may observe, that Dance, the lamented author of the article Auscultation, in the second edition of the Dictionary in twenty-one volumes,³ in speaking of the crepitous rattle says, that it is found only in a single case, that of pneumonia at the first degree, of which it forms the pathognomic sign. But, in speaking of the subcrepitant rattle, he mentions œdema as one of the affections in which it exists. "A little bronchophony is found, particularly at the root of the lung. But the long persistence of the crepitant rattle, and the absence of the general signs of inflammation, enable us almost always to distinguish œdema of the lung from pneumonia at the first degree, even where these diseases co-exist."⁴

It is evident that the therapeutic indications in this disease are those which arise in other forms of drop-sical effusion. Whatever means may promote the absorption of effused serum, must be of peculiar importance when this fluid is thrown out in the midst of

¹ Auscult. Méd. I, 341.

² Ibid.

³ Dict. de Méd. ou Rep. General des Sciences Médicales. (Paris, 1833).

⁴ Auscult. Méd. I, 341.

organs so essential to life. Thus, although this affection is by no means one of those which from their frequency, their intrinsic gravity, and the striking character of their physical signs, would be chosen to demonstrate the utility and importance of direct exploration, yet as its symptoms are obscure, its signs capable of materially assisting us, and the indications for its treatment different from those of diseases with which it might be confounded, it illustrates the value of the information to be derived from the methods of examination whose usefulness we are discussing.

Pulmonary Apoplexy.—Gangrene of the Lung.

The utility and importance of direct exploration, are comparatively limited in both these affections. This is because both of them have certain symptoms which must to a great degree prevent them from being confounded with other affections. In pulmonary apoplexy, the existence of copious hæmoptysis will alone afford the principal indications for treatment. However interesting it may be to explore the condition of the lungs in such cases, it could add little to direct us in the course we are to pursue. On the other hand, if there exist an apoplectic centre in the lungs without hæmoptysis, the diagnosis will often be obscure and probably in some cases impossible. Laennec mentions as the physical signs, flatness on percussion when the effusion of blood is extensive, absence of respiration, and the crepitous rattle at the edges of the diseased parts.¹ M. Piorry, in

¹ Auscult. Méd. I, p. 372.

three cases to which we have alluded before, found a circumscribed flatness on percussion, but nothing by auscultation worthy of notice.¹ M. Cruveilhier declares that auscultation and percussion have taught him nothing in this affection.² His twenty-third proposition begins as follows. "The diagnosis of pulmonary apoplexy presents very great difficulties in the greater number of cases."

In gangrene of the lung, the characteristic fœtor of the sputa is in itself a substitute for almost all other signs.

Chomel has observed the odor of phosphureted hydrogen, or of anatomical objects undergoing maceration, not only in this affection, but also in pneumothorax with communication between the bronchiæ and pleura.³ We shall see, in treating of the latter affection, that its physical signs are very distinct from those of gangrene of the lung. Dr. Gerhard mentions, also, certain cases of bronchitis in which the sputa are fetid, and in which the rattle occasioned by mucus copiously secreted into the bronchiæ, might be mistaken for gurgling.⁴ Notwithstanding these exceptions, the true gangrenous odor is so far satisfactory evidence of the existence of gangrene in the lung as to form almost, if not, as Louis has said, absolutely a characteristic symptom. The physical signs, according to Dr. Gerhard, are a mucous or sub-crepitant rhonchus, without alteration of the resonance

¹ *Auscult. Méd.* I, p. 373, note.

² *Dict. de Méd. et Chir. Art. Apoplexie.*

³ *Lectures.*

⁴ *Diagnosis of Diseases of the Chest*, p. 98.

on percussion, followed by gurgling, cavernous respiration, and pectoriloquy. The sound on percussion may become obscure over the affected part, or if a large cavity is formed, extremely resonant, as in a case mentioned by the same author.

In these two affections, which we have classed together, the use of direct exploration is, as we have seen, confined within narrow limits, yet is not to be entirely rejected, as without practical utility. With regard to the first, we are inclined to believe that subsequent observation will establish its physical signs with greater certainty. In the second, which has a symptom so far constant and characteristic, the study of the physical signs must always be, to a considerable extent, the indulgence of curiosity on the part of the pathologist.

Emphysema.

The term Asthma was formerly applied to a number of affections, different in their nature, but characterized by some peculiar symptoms, particularly fits of difficult breathing. But how loose was the connection between many of the diseases thus classed together may be seen by looking over Sir John Floyer's Catalogue of "Diseases which produce the Asthma as a Symptom."¹ They are, 1. Suppression of the menstrual or hemorrhoidal evacuation. 2. Plethora. 3. Polypi in the heart and lungs. 4. Coagulation of the chyle in the lungs. 5. The obstruction caused by "the viscid serum in a pneumo-

¹ A treatise of the Asthma. London, 1717, p. 96.

nia," &c. The author observes that "these pneumonic spitting asthmas have been observed upon dissection to have tubercula or schirrosity in the lungs; and they frequently turn to an abscess, and that into an empyema."¹ On the next page he says, "but all these asthmatics usually die consumptive." 6. Stones in the lungs. 7. Pica and other cachexies. 8. A long catarrh. 9. Fevers, small-pox, &c. 10. Vomica. 11. Every external compression of the lungs. 12. "Tumors of the viscera which produce a spurious asthma, as that of the liver, spleen, kidneys, pancreas, and all hydropical tumors." He then goes on to describe those symptomatic asthmas which succeed cephalic diseases. Later authors than Floyer seem to have confounded different diseases under this title. Such was the case with Cullen, who unquestionably mistook the cause for the effect when he said, "In some young persons it has ended soon by occasioning a phthisis pulmonalis. After a long continuance it often ends in a hydrothorax; and commonly by occasioning some aneurism of the heart or great vessels, it thereby proves fatal."² Similar notions are expressed by Dr. Good,³ and they are criticised in a similar manner by his editor, Mr. Samuel Cooper, who was acquainted with the labors of Corvisart and Laennec.

It is probable that errors of much consequence to the patient, are, even at the present day, committed, from a want of proper acquaintance on the part of practitioners with the history and signs of *Emphysema*, or dilatation of the pulmonary vesicles, the most frequent cause of Asthma. M. Louis mentions a case, in his

¹ Ibid. p. 98.

² First Lines, par. 1387.

³ Study of Med. I, 430.

lectures, in which he was consulted by a patient in whom the existence of tubercles had been so much apprehended, that he was on the point of changing his residence, in the hope of arresting the disease, which was discovered by a careful examination to be emphysema. It is proper, then, since its symptoms have been, and may be taken for those of other affections, to devote a little attention to its physical signs and their value. These are, first, unnatural prominence of the chest, shown by *inspection*. A good instance of this may be found in the sixth observation of Laennec. M. Louis is in the habit of examining his patients in the erect posture, opposite a window, taking great care that they stand in a perfectly even manner, with both arms disposed alike. By means of these precautions, the most delicate degrees of difference in the formation of the two sides may be detected. The parts particularly to be examined are the sternal edge of the cartilages of the ribs, the regions above and below the clavicles, and generally, the anterior parts of the chest. If any inequality is perceived in the region covered by the pectoralis major, we must examine if there is any difference in the size of these muscles on the two sides.

It is to be remembered that the left side of the chest is apt to be a little more prominent than the other, a circumstance which had struck me in the examination of some cases of pneumonia, presented to a society,¹ at which time I mentioned it, and one which has been since remarked upon by M. Woillez in an inaugural thesis. As the prominence was in the

¹ Société Médicale d' Observation, Paris.

cardiac region, it may have been in some degree the consequence of the heart's action.

It is very possible that the natural difference of formation in the two sides of the chest, may account for the contradictory facts observed by M. Louis, that emphysema was equally frequent in both lungs, but that thoracic projections were about twice as common on the left side as on the right.¹

At the same time that the parts corresponding to the seat of emphysema are more prominent, they return a clearer sound on *percussion*. This is sometimes almost tympanitic.

The *respiratory murmur* at the same points is feeble; it is not uncommon to hear a sibilant or sonorous rattle, sometimes in the form of a prolonged musical vibration.

I have repeatedly observed, in cases of emphysema, that the respiratory sounds offered no interruption, but were continually going on, as if the expulsion of the air were kept up during the intervals of inspiration and expiration by a mechanism like that of a double bellows. In such cases I have heard a continuous musical sound in the chest. It must be in these circumstances that some power in the lungs themselves, either of mechanical or vital contraction, operates to expel the air while the thoracic parietes are at rest, or that the rarified air in the obstructed vesicles escapes by its own elasticity during the intervals of inspiration and expiration.

Laennec considered the dry crepitant rattle with large bubbles as pathognomic of this form of emphy-

¹ Sur L'Emphysème, p. 197.

sema, when heard at considerable intervals, for some instants, and in a limited extent. In some very thin subjects he even felt crepitation on pressing with the finger.¹ Dr. Townsend has found the rattle in question repeatedly, both in vesicular and interlobular emphysema; but at other times it has been wanting.²

M. Louis remarks in his lectures, that the emphysematous patients are commonly suffering, at the time they enter the hospital, with the complication of catarrh; and that in these cases it is usual, as in other cases of bronchitis, to find a sub-crepitant rattle at the lower and posterior part of both lungs. M. Bouillaud remarks, that it is somewhat suprising Laennec has not mentioned among the signs of vesicular emphysema, the sound of friction (or movement of ascent and descent).³ He considers this fact established by the observations of M. Reynaud. Dr. Townsend has witnessed the same phenomenon.⁴ Sometimes the hand applied to the chest, can perceive this movement of friction; and according to M. Reynaud, it may be heard by the patient or an observer at a certain distance.⁵

The signs which we have mentioned, if well characterized, could not permit us to confound the disease a moment with any other excepting pneumothorax. But in this last affection, we must remember that there is almost universally pleuritic effusion; the tympanitic sound is far more intense in the part occupied by the air, the respiration is totally absent, and there is no rattle at the same points. If the pneumotho-

¹ Auscult. Méd. I, 297, etc.

² Lond. Cyc. Art. Emphysema.

³ Dict. de Méd. et Chir. Art. Emphysème.

⁴ Lond. Cyc. etc.

⁵ Dict. Ibid.

may be from perforation of the lung, as in the great majority of cases, the suddenness with which the accidents come on, and the existence of the amphoric respiration, or the metallic tinkling, will render it impossible to confound the two affections. The real difficulty of distinguishing emphysema, is in those cases where the lesion is but slightly marked, and at certain parts of the lungs, as at the diaphragmatic or mediastinal surfaces, it must elude the methods of direct exploration.

Still, it is useful and important in medical practice, to know that in many cases an examination of a few moments, may decide by positive evidence, that symptoms which have occasioned distress and alarm, are owing to a lesion, of which it is true we can only palliate the symptoms and perhaps delay the progress,—but a lesion which is not inconsistent with long life, and above all which does not set upon the patient and his children, the seal of the tuberculous cachexy.

Interlobular emphysema, is considered by Laennec as a traumatic lesion, developing itself instantaneously, and due most frequently to forcible and long continued retention of the air inspired in continued efforts.¹ He remarks that the dry crepitous rattle with large bubbles, is always more pronounced in this than in the vesicular variety of emphysema. The sound of friction belongs also to this variety. M. Bouillaud considers the distinction of these two affections difficult, when there is projection of the air-cells in the vesicular form; but he adds, “happily this distinction is of very little practical importance.”

¹ Auscult. Méd. I, 324-27.

Bronchitis—acute, chronic.

Most physicians have probably seen cases in which, at the time when they were called upon to direct the treatment of a patient, they were unable to say whether the substance of the lungs, or only the bronchial membrane was affected with inflammation. I have repeatedly met with cases, where the symptoms were so far from decisive, that it was necessary to form an opinion principally from the physical signs. An instance of this kind occurred to my notice not long ago. The patient, a stout boy, had been complaining for some days ; but little attention had been paid to him, and he had been doing his best to move about and take food as usual. I saw him in company with an intelligent physician, who had paid little attention to the arts of direct exploration. The boy had coughed, there was considerable febrile movement, with some pain in the chest ; the sputa had not been preserved, and he did not cough in our presence. My friend, whose opinion was formed from the general symptoms, supposed the patient to be affected only with bronchitis, to which of course he attached little importance. A moment's examination by percussion and auscultation, showed that one of the lungs was not only inflamed, but that hepatization had already taken place. Active measures were immediately employed, and followed by relief and recovery. The characteristic sputa, which were observed a day or two after the examination referred to, left no doubt, if such could have existed, of the nature of the disease. The danger of confounding intense catarrh with pneu-

monia has been remarked by Dr. Williams,¹ and indeed the loose term, "inflammation in the chest," is employed to shield the inability of practitioners, at one time to distinguish between these two affections, and at another to cover doubts which are still more vaguely multiplied.

The most important results afforded by direct exploration in bronchitis, are negative. "When the catarrh is simple, however intense it may be, the chest resounds well throughout."² In examining the respiration, we may find it absent at certain points, where it returns, however, after the act of coughing has removed the secretion which obstructed the bronchial tube leading to the part.³ The absence of bronchial respiration, and the existence of the sonorous, the sibilant, the sub-crepitous or mucous rattles at the posterior and inferior part of the lungs, commonly on both sides of the chest, are sufficient to reveal its presence. But it is necessary to say, that in a certain number of cases, probably when the disease attacks the minuter ramifications of the bronchiæ, the sub-crepitous approaches so nearly in character to the crepitous rattle, that it requires attention and habit to distinguish them. The certainty with which the crepitous rattle indicates the presence of pneumonia, leads us naturally to refer the solution in doubtful cases entirely to the character of the rattle; and when this is done by those not thoroughly familiar with the two sounds in question, they are liable to fall into error. Dr. Williams even arraigns

¹ Rational Exposition, p. 77.

² Auscult. Méd. I, p. 134.

³ Ibid. p. 136.

the accuracy of Andral on this point, and says very plainly that it may be owing "to his having neglected the efficient clinical instructions of the great inventor of auscultation,"¹—a peculiarity in M. Andral's course, which Laennec mentions with the remark—"this method seemed to me rather singular."² I can add that M. Louis, who believes that Laennec himself sometimes confounded the sub-crepitous and crepitous rattles, very rarely expresses any doubt by which title to designate sounds of this kind; and I believe that any one, with attention and opportunity, may arrive at a similar degree of confidence. But it should never be forgotten, that direct exploration is but one of our means of ascertaining the character of a disease; that in some cases the most penetrating observers are obliged to wait a little for the development of more distinguishing signs or symptoms before they form an opinion; and that this is far wiser than to leap to a conclusion, trusting to the guidance of that *medical tact*, which seems to its possessor a fountain of light, while, like the lamp in a darkened lantern, it illuminates nothing but its narrow receptacle.

Having ascertained the existence of bronchitis, the prognosis is of course more favorable, and the treatment may be much less active than in cases where the tissue of the lung itself is affected. I do not mean to say, that catarrh is never a grave disease; for the *Peripneumonia notha* of Sydenham, was probably only an intense bronchitis, affecting the minuter ramifications of the bronchiæ, and the epidemic influenzas have

¹ Rational Exposition, p. 75.

² Preface to 3d edition of *Auscult. Méd.* p. 12.

sometimes been attended with threatening symptoms and fatal consequences; but notwithstanding the extreme frequency of this disease, under ordinary circumstances it is hardly ever fatal in adults, and according to Dr. Gerhard, rarely so in children, unless it be followed by lobular pneumonia.¹ However much the course of bronchitis may be abridged, and its symptoms mitigated by appropriate treatment, it recovers in the vast majority of cases in spite of neglect and exposure. But the disease with which it is especially liable to be confounded, pneumonia, is one, which calls for the employment of the most active treatment. Therefore, the signs derived from the use of direct exploration, positive and negative, affording an essential assistance in distinguishing bronchitis from pneumonia, are to be considered, with reference to these diseases, both useful and important.

In *chronic* bronchitis, the absence of flatness on percussion, of bronchial respiration and bronchophony, show that the tissue of the lung has not undergone induration. These negative signs, with the presence of sub-crepitant or mucous rattles at the posterior and inferior parts of the lungs, serve to distinguish this disease from phthisis. When we look at the rational signs, we find that it would often be absolutely impossible to distinguish the two affections by means of them alone. Instances may be found in the works of Bayle² and Andral,³ in which the symptoms had all the aspect of those of a tubercular affection,

¹ Op. cit. p. 71.

² Recherches sur la Phthisie Pulmonaire, Obs. 48, 49.

³ Clin. Méd. Vol. I.

and yet the tissue of the lungs was found entirely free from disease. Now although chronic bronchitis may produce death with all the appearances of phthisis, yet the prospects of benefit from treatment are immeasurably greater in the former. Laennec mentions different means as having been successful, among which are repeated emetics, spirituous remedies, and particularly *punch*, the balsamics, and the vapor of tar. If the two affections are confounded, we may on the one hand distress a patient, whose lungs are already disorganized to a great extent, with remedies which may aggravate, but cannot remove his disease, and on the other hand content ourselves with simple palliatives in a disease capable of recovery under the use of certain agents, but one which "far from tending naturally to a cure, on the contrary becomes aggravated in proportion to its duration and the progress of age." It is therefore in the highest degree important to determine whether a disease which presents the symptoms of phthisis is in reality tuberculous or catarrhal. If the physical signs of the first kind of disease are proved absent by repeated examinations, if on the contrary those which belong to the latter are present, however formidable may be the general symptoms, there is still much to be done and to be hoped for.

I will only advert to a single example to enforce the necessity of caution in drawing conclusions from symptoms. The fifty-third observation of Bayle contains a most interesting history of his own disease. It began in 1802, and presented the symptoms of phthisis to such a degree that he himself and the physicians around him looked forward to its speedy

and fatal termination. About two months from its commencement there took place a violent chill followed by the most profuse sweating. A few days after, he was convalescent, and in a month from the apparent crisis his health was entirely re-established. From that time until 1810, when the work which contains this observation was published, he had no symptom which resembled phthisis, and was then enjoying good health. The observation is entitled "Chronic pulmonary catarrh, having all the appearances of pulmonary phthisis, cured spontaneously." He died soon after, of pulmonary phthisis.

In looking over the symptoms, it is rational to suppose that the phenomena which he has recorded were due to the same lesion which terminated his life so many years afterwards, but as there is no mention of the employment of percussion, the only method of direct exploration then practised which would have afforded positive information, we have only the presumption afforded by the history of the case.

Dilatation of the Bronchiæ.

It is not very uncommon to find a slight dilatation of the bronchiæ in patients in whom it had not been suspected during life. Much more rarely, a patient who had presented the symptoms of phthisis, is found on examination not to have been tuberculous, but affected with the lesion in question. The disease being an organic change not susceptible of cure, and capable of producing death with the general symptoms of phthisis, it is a matter of less consequence

to distinguish these two affections than many others. And it is in this very case that the physical signs are most apt to be insufficient to establish the diagnosis. For in both affections there may be formed extensive cavities, which shall give rise to similar phenomena of the voice and respiration. Percussion generally affords a flat sound over a tuberculous cavity, due to the condensation of the pulmonary tissue around it. In dilatation of the bronchiæ, the compression of the pulmonary tissue may, according to Laennec, diminish the sonorousness on percussion, but this sign, he says, is commonly little marked.¹ Dr. Williams² mentions the regions principally affected in this disease as the scapular, mammary and lateral,³ while, as we have seen, the seat of tubercles is most commonly about the subclavian and acromial. The absence of flatness on percussion to any considerable degree, and the situation where the signs of disease are found, may sometimes enable us to draw the distinction between these two diseases. In the only case of fatal dilatation of the bronchiæ which I remember seeing in the wards of la Pitié, M. Louis had long recorded a correct diagnosis, founded in a great measure on these circumstances. The eleventh observation in his work on phthisis, entitled "Dilatation of the Bronchiæ at the summit of the Lungs, taken for a tuberculous excavation," is one in which the diagnosis must have been very difficult; but even in this instance the perfect sonorousness under the clavicles left some doubt in the observer's mind, as it seemed probable to him

¹ Auscult. Méd. I, p. 203.

² Lond. Cyc. Art. Bronchitis.

³ See 'Rational Exposition.'

that a tuberculous disease of so long standing would have produced some induration, and consequently flatness on percussion. The mistake was committed however; it may be committed again, but it cannot be very often; for, as Louis remarks, such cases are rare, and many years will probably elapse before the observer will meet with one entirely resembling it. I recollect a patient in whose case M. Louis had long hesitated whether to consider the affection as dilatation of the bronchiæ or tubercles. He was much inclined to favor the first supposition, from the existence of the physical signs of disease at the posterior and inferior parts of the lungs. The patient proved to be affected with tubercles, but it was one of those exceptional cases, in which the disease had been more fully developed in the lower than in the upper parts of the lungs.

Since commencing this dissertation, an intelligent physician from a neighboring town mentioned to me a case in which he and his brother auscultators had diagnosticated tuberculous disease, although the physical signs were not found in the common region of this lesion; but on examination, they discovered the disease to be dilatation of the bronchiæ.

We are justified then in insisting upon extreme caution in giving an opinion in doubtful cases of this nature; not so much on account of the patient, who may die of either affection, and can hardly be cured of one or the other, as for the sake of the physician, and the art, which suffer from all such errors.

We conclude that direct exploration is useful in this affection, because it may enable us to distinguish it from phthisis; a disease far more certainly fatal; and

at any rate is capable of revealing the presence of the cavities formed by the dilated bronchiæ, thus showing the patient to be affected with a lesion which we must not expect to overcome, like chronic catarrh, by the use of remedies, but which may probably remain stationary for a long time, or through life.

DISEASES OF THE PLEURA.

Pleurisy.—Empyema.—Pneumo-thorax.

We arrange these diseases together for these reasons. The notion that *Empyema* is formed by a collection of matter which has escaped from a vomica, is entirely exploded. A tubercle or a tuberculous cavity does indeed not unfrequently open into the pleura, but the collection of matter which follows in the chest is the consequence of the pleurisy excited by the presence of the tuberculous matter, or of the air, which act as irritating foreign substances upon the serous membrane. *Pneumo-thorax* is almost always owing to a similar cause, and consequently complicates pleurisy.

As the general notion attached to pleurisy is that of a pretty well characterized disease, it may not be uninteresting or useless to glance at the history of its diagnosis, as found in the works of the ancients and moderns. In the first place, the ancient writers appear to have confounded the disease with pneumonia, by the characters they assign to the sputa, which are, according to several of the most distinguished authors, "bilious," bloody, yellowish, viscous, greenish, or

blackish.¹ At other times they appear to have confounded it with hepatitis, the distinction between which and pleurisy is much insisted upon by Trallian and Paul of Ægina,² and may be seen illustrated in the story told by Galen, and known as "*Hepatici cognitio*."³

Aetius devotes a chapter to an affection resembling pleurisy, the consequence of indigestible food, ("*De ea quæ pleuritis esse putatur quum tamen non sit*."⁴) which he says has led to fatal errors. "*Unde quidam medici errore seducti, vena cubiti incisa multoque sanguine evacuato, mortis causam ægris præbuerunt*." Among the authors of more modern times, Boerhaave tells us that the fever is sometimes masked by the difficulty of respiration, "*unde sæpe turpiter fallitur medicus*."⁵ Baglivi attributes the greatest consequence to the kind of pulse as a diagnostic sign, and even begins his chapter on pleurisy with these words — "*Si vis cognoscere pleuritidem, præcipuam curam in natura pulsûs reponito; pulsus durities est signum ferè infallibile omnium pleuritidum*."⁶ And it is on the very next page, and speaking of this same affection, that he makes the often quoted exclamation on the difficulty of treating, and the still greater difficulty of diagnosing and foreseeing the issue of diseases of the lungs. With such tests as he relied upon, well might he acknowledge, "*Fallunt vel peritissimos ac*

¹ Vide Aretæum de Caus. et Sign. Acut. Morb. Lib. I, Cap. X. Alex. Trallian. Lib. VI, Cap. I. Paul. Ægin., Lib. III, Cap. 33.

² Loc. Cit.

³ De Locis Affectis, Lib. V, Cap. 7.

⁴ Tetrabibl. II, Sermo. IV, Cap. 69.

⁵ Aphorism, 882.

⁶ Prax. Med. Lib. I, Cap. 9.

ipsos medicinæ principes." One of the authors quoted in the *Sepulchretum* of Bonetus, tells us that in many post mortem examinations of patients supposed to have been affected with pleurisy, he has found the *lungs* with the pleura investing them in a state of disease, and therefore concludes that pleurisy is rarely solitary.¹ No better commentary on the deficiency of the long established diagnostic signs of pleurisy can be found than in the commencement of the sixth chapter in the second book of Cullen's *First Lines*; "Of pneumonia, or pneumonic inflammation." "Under this head I mean to comprehend the whole of the inflammations affecting either the viscera of the thorax, or the membrane lining the interior surface of that cavity; for neither do our diagnostics serve to ascertain exactly the seat of the disease; nor does the difference in the seat of the disease exhibit any considerable variation in the state of the symptoms, nor lead to any difference in the method of cure." In the *Memoires de l'Acad. des Sciences* for 1789, is a memoir of M. Portal, entitled, "An observation proving that pleurisy is not a disease essentially different from peripneumony."²

One would have supposed that the presence of fluid in the pleura would be denoted by symptoms which could hardly admit of mistake. But experienced observers have supposed that there was fluid in the chest when there was none, and overlooked it when actually present. "Sed quæ tamen, ut veræ sunt, ita non faciunt, quin illæ pariter veræ sint quas opposuit Reimannus, eaque præsertim quæ medicum

¹ Vol. I, p. 620.

² Laennec, II, 283. Note.

exercitatissimum Jo. Jacobum Vicarium eo adduxerat ut ob istud imprimis signum, *jurare* se posse, crederet, *infallibiliter ægrum hydrope pectoris laborare*, cujus mortui aperto thorace cum in dextero, sinistroque hujus cavo *nec drachmam unam aquæ ; aut seri inveniret ;* nunquam satis laudanda ingenuitate mirabundus exclamavit, *quam fallacia sunt subinde diagnostica !*"¹

"But it is rare that all these symptoms coexist ; commonly a greater or less number of them are wanting, and sometimes there are scarcely any. The diagnosis is then difficult or even impossible. Consequently, there are many examples of *empyema* which have only been recognised at the examination of the body after death, and whose existence had not even been suspected during life."² "The opposite error is more difficult and more rarely committed ; still there are examples of persons supposed affected with empyema, and in whom an opening has been made into the chest without finding any fluid in this cavity. Dionis informs us that in his time a surgeon, and a skilful one, practised the operation for empyema upon the duke of Mortemart, and found nothing in the chest."³ The author goes on to say that the result of this operation was not mentioned, but that we can conceive it might have dangerous consequences in a disease which could be taken for empyema. I have been informed, upon the highest authority, that a similar operation was performed many years ago by a practitioner, then celebrated in this part of the country, upon a patient laboring under phthisis, under the

¹ Morgagni, de Sed. et Caus. Morborum, Epist. XVI, P. 11.

² Boyer, Traité des Mal. Chirurg. Tom. VII, pp. 369, 370. (4th edit. 1831.)

³ Boyer, Op. Cit. pp. 371, 372.

vague notion of getting rid of matter contained in the lungs. It so happened, that he opened upon their most healthy portion, so that collapse of the lung and pneumo-thorax must have been the immediate consequence. Symptoms of the most threatening nature instantly declared themselves, and death occurred in a very few days. Accident is sometimes kinder than Art. "E diverso Phalereus tum deploratus à medicis vomicæ morbo cùm mortem in acie quæreret, vulnerato pectore medicinam invenit ex hoste."¹

I have, perhaps, occupied too much space to show the insufficiency of the general signs to distinguish pleurisy, and its consequence, empyema, with certainty. Let us now examine the physical signs which characterize the different changes that take place in the cavity of the pleura in consequence of or in connection with inflammation. These consist in the presence of false membranes, the accumulation of fluids, liquid or gaseous, and in certain changes which follow the absorption of these fluids.

The most remarkable sign which indicates the existence of false membranes on the pleura, is the *bruit de frottement*, the motion of ascent and descent, or, as Dr. Forbes translates it, the sound of friction. This sign, first observed by M. Honoré, and attributed by Laennec to interlobular emphysema,² has been since found by M. Reynaud³ to exist in this condition of the parts in pleurisy; a fact often since confirmed by other observers.⁴ I remember a patient in whom it existed a considerable time, and was attended by a

¹ Plinii Hist. Mundi, Lib. VII, Cap. 50.

² Auscult. Méd. I, 119, 120.

³ Ibid. Note.

⁴ Vide Auscult. Méd. II, 319. Note.

consciousness, on his part, of the jerking motion which accompanied the sliding of the roughened laminæ of the pleura over each other.¹ The other lesions which are sometimes accompanied with this sign, vesicular and interlobular emphysema, being characterized by symptoms very different from those of pleurisy, will rarely be confounded. There ought, in these cases of *dry pleurisy*, as they have been called, to be a certain degree of obscurity upon percussion, perhaps some modification of the respiration and voice, but it is more common to find liquid effusion at the same time that there exists membranous exudation.

For the diagnosis of pleurisy with fluid effusion, direct exploration offers more resources than for that of any other affection. All its different methods in fact are applicable.

Inspection reveals to us either the dilatation of the chest or its immobility. The first circumstance had been observed by Hippocrates. "Quibus latus sublevatum in tumorem ac calidius est—his pus ex una parte est."² Laennec remarks, that this has been observed by all the authors who have treated of empyema, but adds, from his own observation, that it exists in recent pleuritic effusion.³ He even says, "I have seen the chest manifestly dilated in the space of three hours"⁴ (from the invasion).

The *immobility* of the chest, in a case of pleurisy

¹ Since this dissertation was submitted to the committee, I have seen a case in which the sound of friction was perceptible at the foot of the patient's bed, or at a distance of about five feet.

² *Coacæ Prænotiones*, Vol. I, p. 564. Edit. Vander Linden.

³ *Auscult. Méd.* II, 318.

⁴ *Ibid.* p. 292.

with effusion, is thus mentioned by Stoll. "Thorax dexter in inspirando vix attollebatur, cum totum ferme respirationis negotium perageretur pulmone sinistro; ubi dexter thorax levissime solum, et nonnisi ob nexum cum thorace sinistro, modicissime elevaretur. Hæc inequalis utriusque thoracis elevatio et visu poterat observari, et imposita utraque ad utrumque thoracem manu."¹

Mensuration, which is only mediate inspection, is a means of verifying the apparent dilatation of the chest rather undervalued by Laennec, who says that half an inch of difference in the circumference of the two sides of the chest is sensible to the eye, and that when the difference is less than this we cannot trust to the accuracy of our measurement.² To render this method more exact, M. Chomel makes use of a measure like that of shoemakers, which, like many other cumbrous contrivances, will probably be only employed by the inventor.³

Palpation, like inspection, shows us the dilatation of the affected side, if it be considerable, or the fluid tends outward—"prominens collectio sentitur"⁴—and the immobility of the affected side. We may learn by this means also the change of position of the heart, by its pulsations heard out of place, and the depression of the liver or spleen, by feeling them beneath the ribs—circumstances which happen in very copious effusion.⁵ M. Reynaud first called the at-

¹ Ratio. Med. Part III, p. 105, 6.

² Auscult. Méd. I, 19.

³ Vide Piorry, Procédé Oper. p. 65.

⁴ Cælius Aurel. Acut. Morb. 6, Lib. II, Cap. 17. (Pleuritica passio.)

⁵ Vide Piorry, Op. cit. p. 67.

tention of observers to the absence of the natural thrill as perceived by the hand upon the chest while the patient is speaking, at the part corresponding to the effusion.¹ Fluctuation may sometimes be detected,² which must be carefully distinguished from that of a superficial abscess.³ Senac, Corvisart, and Pinel had observed an undulation in the intercostal spaces, which neither Laennec nor M. Piorry, however, were able to detect in the cases they witnessed.⁴ The sign discovered by Mr. Tarral, and called by him "fluctuation peripherique"⁵—peripheric or superficial fluctuation, might be advantageously sought after by employing the process which will be mentioned hereafter.

Succussion, or the sudden agitation of the patient's body in order to produce the sound of the liquid in motion, is a physical sign long recognised as useful and important. Among the ancient authors, it was repeatedly mentioned by Hippocrates,⁶ as also by Galen,⁷ by Aetius,⁸ and among the Arabian authors by Avicenna.⁹ Laennec speaks of several more modern authors who relate cases in which they heard the sound of fluctuation during the spontaneous movements of the patient, among whom are Ambrose

¹ Laennec, II, 319. Note.

² Warner's Cases in Surgery, cases 31, 32.

³ Lond. Cyc. Art. Empyema, (Dr. Townsend,) Art. Pleurisy, (Dr. Law.)

⁴ Piorry, op. cit. p. 65.

⁵ Ibid.

⁶ De morbis II, 45. Coacæ prænot. (Vol. I, p. 565, Ed. Vander Lind.)

⁷ De Locis affectis, Lib. IV, Cap. 8.

⁸ Tetrabiblos, II, Sermo. IV, Cap. 65.

⁹ Lib. Canonis, III, Fen. 10, Tract 4.

Paré, Willis, and Morgagni.¹ Mr. Samuel Sharpe, as is stated by Cooper,² had observed this phenomenon. He compares the sound, as did Paré also, to that produced by shaking a vessel half full of water. But notwithstanding such ancient and respectable authorities; notwithstanding that Laennec had observed it himself nearly forty times,³ M. Begin complains that this element of diagnosis is too much neglected,⁴ a neglect probably owing to the more constant presence of the signs afforded by auscultation and percussion. In fact, in order to produce a sound upon succussion, the effusion must be in considerable quantity, and some aeriform fluid must exist in the cavity of the pleura at the same time. This coincidence is not found every day in walking through a hospital, but on the other hand it is not so rare as might have been supposed. I have myself seen two cases at least in which it existed, one of which will be again alluded to.

Percussion.—It is a matter of wonder that the old physicians and surgeons, who would “swear,” as Morgagni says, that there was fluid in the chest, when in reality there was not “a single drachm,” or perform paracentesis of the thorax upon a duke for an empyema which did not exist, and at other times overlook the existence of large effusions in the pleura, should not even by accident, and particularly in examining the bodies of their unfortunate patients, have

¹ Auscult. Méd. II, p. 443.

² Surgical Dict. Art. Empyema.

³ Auscult. Méd. II, 449.

⁴ Dict. de Méd. et Chir. (Art. Empyeme.)

observed that the side of the chest containing fluid, on receiving an accidental blow, did not return the natural clear resonance. When I have seen an ignorant patient detect by her own sensations the vibratory thrill occasioned by the voice in an indurated part of the lung, a sign which has only been known to medical observers for a few years, it has seemed unaccountable, that phenomena so much more striking, should have remained so long unnoticed.

But the first application of percussion to diseases of the chest, upon record, is to be found in the *Inventum Novum* of Avenbrugger. Among the few writers whose attention was attracted by his invention, was Stoll. In a case of pleurisy reported in the *Ratio Medendi*,¹ remarkable as being perhaps the best case with regard to the exact description of the physical signs before the days of Laennec, he says, "*Thorax quoque dexter methodo Avenbruggeri pulsatus, illo sonitu caruit, quem aliàs sana thoracis cava, præscripta methodo pulsata, edunt.*" The Treatise on mediate auscultation brought this method of exploration into notice, and its utility in pleurisy with effusion, is now so familiar to most well educated physicians, that it is hardly necessary to bring forward evidence to prove its constancy. Laennec, who showed a tendency to underrate the value of percussion, compared with his own invention, auscultation, and M. Piorry, who makes a graven image of his pleximeter, have estimated differently the relative importance of the two methods.² I do not think it

¹ Part III, p. 106.

² *Auscult. Méd.* II, 311. *Procedé Op.* p. 59, etc.

necessary to attempt to settle the rival claims of two modes of exploration, which should be used to assist each other, and considered also in the light of the general symptoms. Laennec himself remarks, that although the absence of sound on percussion might be owing to pneumonia as well as pleurisy, yet the general and local signs will enable us to distinguish them.¹ On one point it appears to me evident that Laennec is wrong, and M. Piorry right,—with reference to percussing the patient in different positions, and thus demonstrating the existence of a fluid by the change of level which must take place, and be rendered plain by the variations of the sound in the same points of the chest. I certainly remember an instance, exceedingly obscure in its symptoms, and in which I was unable to come to a conclusion, until this experiment settled the existence of an effusion. Laennec attaches more consequence to the great extent and rapid supervention of the flatness upon percussion. “Not unfrequently,” he says, “after some hours of disease, the sound is flat all over the side affected, or in its lower half, which never, or almost never happens in pneumonia.”²

With regard to the quantity of fluid which it is possible to recognise by percussion, M. Piorry assures us that he is able to detect two or three ounces in the right side of the chest, and that if the natural sonorousness is wanting over the lung between the spleen and the spinal column, it is sufficient to excite suspicions of a pleuritic effusion ;³ mentioning a case in the same connexion, in which he announced a

¹ Loc. cit.

² Auscult. Méd. II, 312.

³ Procédé Op. p. 69, 70.

diagnosis of this kind, which was verified, notwithstanding the incredulity of his students. I am willing to allow that the refinements into which the ardor of prosecuting a new invention has carried M. Piorry, are too subtle in some instances for senses of common acuteness and common education. But that any considerable quantity of fluid could not exist without manifestly changing the resonance on percussion, unless it were bounded by adhesion to the space between the lung and the diaphragm or mediastinum, or between the lobes, is so evident from *a priori* considerations, that the authority of Avenbrugger and Stoll, of Bayle and Corvisart, and the innumerable confirmations which this fact has received from Laennec and his successors, are an unnecessary luxury of evidence, except for those whose scepticism must be combated by the weight of illustrious witnesses.

When, in consequence of tubercles or gangrene, a communication has been formed between the bronchiæ and pleura, there is a short interval during which the disease might be considered simple pneumo-thorax. But as the irritation produced by the foreign substances and the air, which suddenly come in contact with the serous membrane, is a necessary cause of pleurisy, within a very short space of time the pneumo-thorax becomes complicated with effusion of liquid. In the first transient state, the sound on percussion is of course tympanitic in the side affected, and the signs offered by auscultation, with the sudden supervention of the peculiar symptoms attending this accident, can leave no doubt of its nature. When there is liquid effusion at the same time, the region which it occupies is flat on percussion, while

the tympanitic sound is obtained over the parts occupied by air. In the cases where aeriform fluids are developed by an effusion of long standing, the same signs will be found on percussion, but the character of the symptoms, with the absence of some auscultatory phenomena, will draw the line of distinction.

Auscultation.—Laennec considered the diminution or suppression of the respiration, and hægophony, or the bleating character of the vocal resonance, as the signs of pleuritic effusion derived from this method of exploration.¹ Andral remarked in certain cases, the existence of bronchial respiration and a vocal resonance, not very dissimilar to bronchophony.² Cruveilhier, who applies the term “tubaire”—tubular,—to the respiration, voice, and cough, both of pleurisy and pneumonia, remarks, that the respiration and voice are jerking (*saccadés*) in the former, while they are full and distinct in the latter;³ one of the principal characters by which Laennec distinguished hægophony from bronchophony.⁴ All these physical signs are observed in different cases. Laennec affirms that hægophony is never wanting at the commencement of pleurisy in a patient in whom the serous membrane was previously free from disease.⁵ He declares that he has found it in all the cases which he saw during five years, excepting such as had been chronic and were tending to recovery, or such as were very slight; and that he had even found it where there was effusion of only three or four ounces.⁶ He remarks

¹ Auscult. Méd. II, 312.

² Clin. Méd. Vol. II, 609—12.

³ Dict. de Méd. et Chir. Art. Pleurisie. ⁴ Auscult. Méd. Vol. I, p. 70.

⁵ Op. cit. Vol. II, p. 320.

⁶ Ibid. Vol. I, p. 73.

on the same page, however, that this phenomenon ceases as soon as the effusion becomes very abundant, and especially when copious enough to produce evident dilatation. Thus this characteristic sign ceases just at the time that the results of inspection and percussion become so marked as to render it unnecessary.

In the complication with pneumo-thorax, the respiration and vocal resonance are wanting at the points corresponding to the collection of air, as well as those corresponding to the liquid; the lung being compressed backward so that only traces of respiration are to be found at its root, where hæmophony may also exist.

If there is communication between the pleura and bronchiæ, the sound called by Laennec, "bourdonnement amphorique," or "utricular buzzing," may be heard. The metallic tinkle is found in the same circumstances; but this phenomenon may exist without the communication of the bronchiæ and pleura.¹ The last sign may be met with in one other circumstance—the case of a very large tuberculous excavation.

While these phenomena are found on the affected side, the lung of the other side being obliged to perform its function with increased activity, its respiratory murmur acquires greater force, and becomes like that found in children, whence it is called puerile respiration. Pleurisy is not attended with rattles, unless accompanied with some other affection. Although it is not uncommon in cases of pneumonia to find a layer of coagulable lymph on the pleura, yet according to Chomel,² this secondary pleurisy does not constitute

¹ Auscult. Méd. Vol. I, p. 113, etc.

² Lectures.

a serious complication, and it is rare to find any considerable liquid effusion co-existing with pneumonia. The same author asserts that it is rare to find pleurisy in connection with bronchitis.

The question which presents itself most frequently, is to determine whether a patient presenting flatness upon percussion, on one side of the thorax, is affected with pleurisy with liquid effusion, or whether there is hepatization of the lung.

This question is almost always susceptible of resolution by careful examination. So many elements indeed go to decide the matter, that it is highly probable some among them will prove applicable to the difficulty. Thus, if percussion return a flat sound over one whole side of the chest, or to a great extent, and the general symptoms be very slight, this is, according to Chomel, evidence of the existence of pleurisy. Some signs are in themselves characteristic; as the dilatation and immobility of the chest, the sound upon succussion, the sense of fluctuation obtained in the common or more novel method, or the presence of distinct hægophony. On the other hand, in pneumonia, its characteristic rattle may almost always be obtained by making the patient cough; and the bronchial respiration and voice are found on a level with the part flat upon percussion.

There is an exceedingly rare disease which may from time to time lead even a careful observer to announce the existence of pleurisy. I remember the case of a boy in one of the wards of M. Louis, who presented great dilatation of one side of the thorax, with entire flatness on percussion, and absence of the respiratory murmur. It was supposed that all these signs

were occasioned by the effusion of fluid, and that the patient was suffering under chronic pleurisy complicating a tubercular affection. The disease proved fatal; and on examination, it was found that the physical signs had been occasioned by an *encephaloid tumor*, which had gradually compressed the lung, and occupied the cavity of the pleura. Similar cases have been observed by others. Corvisart reports one which he witnessed,¹ and mentions two which were seen by Boerhaave and by Lallemand. Perhaps the barber mentioned by Fabricius ab Aquapendente had fallen on a similar disease. "Unde barbitonsor dixit, exire cerebrum, mirabatur enim veluti substantiam cerebri, tanta erat ejus crassities."² Laennec refers to instances of tuberculous and melanotic matter developed in the pleura, and of cysts in one instance between the intercostal muscles and the pleura, and in another occupying both sides of the thorax. But all these morbid productions in the pleura in the shape of voluminous tumors are extremely rare, and the case which I saw was thought worthy of being exhibited to the Academy of Medicine.

To illustrate many of the phenomena found in pleurisy with effusion, I will sketch the outline of a case which I saw a few years since, from notes taken at the time.

** ** Æt. 22, entered a public institution, Oct. 29, 1832. He had been ailing six months, and attributed his disease to a cold. The chief symptoms had been

¹ Translation of Avenbrugger, p. 49.

² Opera Chirurgica, p. 243. (Patavii, 1666).

cough, dyspnœa, soreness in the chest, palpitations, anorexy, uneasiness after food. At his entrance the respirations were 35, the pulse 100 in a minute, the skin warm and dry, the decubitus on the left side. The patient was emaciated, but could sit up, and I think, move about.

The results afforded by direct exploration were as follows :

Inspection.—Very marked dilatation of left side of chest.

Mensuration.—Right side of thorax nearly 16 1-2 inches in circumference, left side nearly 19.

Palpation.—Impulse of heart very strong—felt in right side of chest as well as left.

Percussion.—Flat below level of third rib in sitting posture.

Succussion.—The fluctuation of the fluid in the chest can be heard throughout the apartment, on the patient's moving the trunk backwards and forwards.

Auscultation.—Respiration absent, or almost entirely absent, in the left side of the chest. Hægophony at one time about angle of left scapula.

Paracentesis of the thorax was performed on the 15th of December, and *nine pints* of purulent fluid evacuated. On the 18th another pint, and on the two subsequent days additional quantities were drawn off by introducing a director. On the 20th the patient died. The left lung was found crowded against the mediastinum, its substance solidified and tuberculated. The pleura was covered with false membranes, and contained three or four pints of

purulent fluid, the rest of the cavity being filled with air. The heart was pressed quite into the right side of the chest, and the pericardium contained pus and false membranes.

Partial Pleurisies.

In some of these cases it must be granted that the physical signs are insufficient to decide the diagnosis. Laennec, who passes very lightly over the signs of these affections, says they may be recognised by flatness on percussion, absence of respiratory murmur, or even hægophony, if sufficiently extensive. At the same time he allows that if there were no hægophony, and if there had been no pain in the chest at the commencement, it would be *somewhat difficult* to distinguish a partial pleurisy from a voluminous tumor developed in the lung.¹ In the cases reported by Andral,² where the effusion was between the lung and diaphragm, sensibility to pressure on the side affected, slight depression of the ribs and displacement of the liver downwards, and feebleness of the respiration were the only physical signs. In a case of interlobular effusion related by the same author,³ no physical sign revealed its existence. These affections still remain, then, among the diseases whose diagnosis may present most obscurity. I am not aware that the error of Desault has been repeated, since the invention of percussion and auscultation, but it would be more pardonable than many others. Intending to puncture

¹ Auscult. Méd. II, 375, 76.

² Clin Méd. II, 494.

³ Observ. 23.

the pericardium, he plunged the trocar into a circumscribed empyema at the base of the left side of the thorax.¹

We may remark that the physical signs of *hydrothorax*, must be like those of pleurisy with liquid effusion. We could not expect, however, to find the sound of friction, as in this affection no false membranes are effused. It is important to remember that essential dropsies of the serous membranes are excessively rare.² Hydro-thorax does not commonly make its invasion with striking symptoms, and it is generally found on both sides, so that an effusion being found on one side only, it should be presumed to be pleuritic.³

Contraction of the Chest after Pleurisy.

Laennec first called distinctly the attention of observers to this remarkable phenomenon, investigated its history, and determined its signs. One of the most striking is the evident loss of capacity of the side which has been the seat of pleurisy, shown by inspection or by mensuration, which frequently makes known a diminution of an inch or more. The resonance on percussion is lessened, for two reasons, the contraction of the limits of the cavity of the side, and the presence of thick, adherent false membranes. The respiratory sound is diminished or absent.⁴ Laennec remarks that he has often found this change of form, even in a high degree, in patients who had never perceived it themselves.⁵

¹ Dict. de Méd. et Chir. Art. Empyème.

² Louis, Lectures.

⁴ Auscult. Méd. II, 338.

³ Ibid.

⁵ Ibid. p. 335.

We have seen the obscurity that rested over the diagnosis of pleurisy as late as the days of Cullen and Portal. We have seen that direct exploration offers all its resources to enable us to detect its presence, among which are three phenomena, dilatation of the chest, audible fluctuation, and hægophony, more truly pathognomic than almost any other signs of disease.

It remains to show that the knowledge of its existence modifies our views of the prognosis and treatment.

Contrast the prognosis of this affection with that of pneumonia, as given by two of the most competent observers.

"*Pleurisy* is rather a slight affection, the febrile movement of little intensity, and the secondary phenomena rare."¹

"Scarcely any patients die of pleurisy but such as are tuberculous."²

"*Pneumonia* is of all acute diseases that which carries off the greatest number of mankind."³

The peculiar indications of *treatment* which may result from the diagnosis of a questionable disease as pleurisy, by the aid of the physical signs, are the following.

1. The depleting system will not require to be carried to the extent necessary in pneumonia; yet it will often be employed, which it might not be, if the disease had been confounded with bronchitis or tubercles.

¹ Louis, Lectures.

² Ibid.

³ Laennec, Auscult. Méd. Vol. I, p. 392.

2. All local remedies will be applied with a knowledge of the seat of the disease.—(Cupping, leeching, blisters, stimulant plasters, etc.)

3. The use of diuretics, or purgatives, will be indicated, to get rid of copious effusion.

4. The pain sometimes produced by the friction which gives rise to the *bruit de frottement*, may be prevented by a bandage around the chest. (Louis.)

5. The peculiar utility of a very powerful and dangerous remedy—tartarized antimony in large doses—is not generally recognised in pleurisy, while its employment in pneumonia stands upon the highest authority.

6. No indication is offered for the use of expectorants.

7. The operation of paracentesis of the thorax, an operation which was followed by recovery in eight out of ten cases of simple empyema, performed in London of late years,¹ although coming within the province of surgery, must often be undertaken by the advice of a medical practitioner.

The indications in hydro-thorax are those of other dropsical effusions, but it is obvious that any considerable quantity of fluid in the pleura, must interfere much more with the vital functions than the same quantity in the abdomen or in the cellular membrane, and that consequently it is doubly urgent to produce its absorption.

Contraction of the chest after pleurisy is rather a

¹ Lond. Cyc. Art. Empyema.

deformity than a disease. It is of course beyond the reach of medical treatment, and it is principally important to ascertain in order to guard against errors in the diagnosis of other diseases.

DISEASES OF THE CIRCULATING SYSTEM.

Pericarditis.—Hydro-pericardium.—Pneumo-pericardium.

In looking back upon the history of pericarditis, we cannot but be struck with the manner in which it has been forced to unmask itself under the eye of direct exploration. Morgagni, who endeavors with much industry and patience to determine the symptoms of "Hydrops pericardii," which, as he used the term, included pericarditis, in alluding to the necessity of proving the existence of accumulated fluid in the pericardium, before performing paracentesis, uses these expressions. "Necdum enim, Ars, quantum video, adeo proficit, ut id certissime, et evidenter cognosci queat. Utinam liceret, sæpius repetitis observationibus, si non alia signa animadvertere, at ex iis quæ proposita sunt, quæ sæpius, quæ rarius tum inter initia, tum saltem prope morbi finem, ipsum comitentur, statuere."¹

If we look at some of the most celebrated systematic writers, we shall find this disease left in great obscurity. Thus Cullen says, "An acute inflammation of the pericardium, is almost always a part of

¹ Epist. XVI, p. 49.

the same pneumonic affection I have been treating of; and is not always distinguished by different symptoms."¹ This he says at the end of the chapter on *Peripneumonia Notha*.

According to Dr. Good, Frank and Vogel concur in the testimony of Cullen, and he quotes this sentence from the latter—"Cordis inflammatio feré ut in peripneumonia."² Notwithstanding the attempts of Dr. Good to draw the distinction between pericarditis and other affections, his editor, Mr. Samuel Cooper, remarks, that "the obscurity in the diagnosis of pericarditis, is still generally acknowledged." He quotes Ribes as still asserting that it has no group of distinguishing symptoms. Dr. Gregory, in the course of a single paragraph, has the two following observations. "The diagnosis of pleurisy and pericarditis, is often a matter of difficulty."—"This symptom (anxiety, paleness of face), however, fails as a diagnostic mark between this disease and bronchitis, which has often, I believe, been mistaken for it."³ Dr Armstrong declares that "though nosologists have pretended to distinguish inflammation of the pleura of the left side from inflammation of the pericardium, there are no signs which can be depended on as strictly diagnostic between them."⁴

Thus, the student who shall rely upon these popular authorities, will be able to justify himself in mistaking inflammation of the pericardium successively,

¹ First lines, p. 383.

² Study of Med.—Empresma Carditis.

³ Practice of Physic, Vol. I, p. 444.

⁴ Pract. Illust. of Typhus and other Fevers, etc. p. 27.

for that of the bronchiæ, or of the tissue of the lungs, or of the pleura. And without making a rigorous application of the mathematical proposition, that two quantities which are equal to a third, are equal to each other, we may here remark that the different diseases thus confounded with pericarditis, must have been none too accurately determined as to their characters in the minds of those who confounded them with this affection.

It is singular that Laennec should have left the diagnosis of this disease in so much obscurity. He uses the following language. "I have sometimes seen others *divine* its existence, and I have sometimes *divined* it myself; for I cannot employ the word *recognise* when there are no certain signs, and when it happens as often that we are deceived, as that we are correct." It was reserved for M. Louis, by bringing into light more fully the value of the signs derived from inspection and percussion, to reduce this evasive disease into the rank of those which may be confidently diagnosticated. The additional information afforded by auscultation, has within a very few years been more clearly illustrated by Messrs. Latham, Stokes, and Hope in Great Britain, and by M. Bouillaud in France.

Signs derived from *Inspection*. An unnatural prominence in the præcordial region might have been anticipated on finding the large quantity of fluid frequently effused in the pericardium—from one to four pints in some of the cases analyzed by M. Louis,¹ and from remembering the facility with which the

¹ Recherches Anat. Path. p. 281.

thorax is dilated in copious pleuritic effusion. Besides, it is just at the yielding cartilaginous portion of the ribs that the fluid must exert its pressure. This phenomenon was actually noticed in one of the two cases reported by M. Louis in his essay on Pericarditis,¹ but not being willing to draw a conclusion from a single fact, he noticed the circumstance, leaving it to future observation to determine whether it was an accidental coincidence, or a constant character of effusion in the pericardium. Long after the observations of M. Louis had convinced him that the projection of the walls of the chest was in reality occasioned by the effusion ;—a year after he had mentioned in a public lecture,² eight new cases of pericarditis, all of which presented this projection, it seemed proper to M. Bouillaud to advance this connection of cause and effect between the morbid state and the external sign, as resulting from facts which were “his own property.”³ Whatever claims M. Bouillaud may have in discovery, this certainly does not belong to him ; and I am equally sorry to see him urging so unfair a title, and in another direction to find the picturesque description of the symptoms in disease of the valves of the heart, published long since in the work of Bertin and Bouillaud, transferred to Dr. Hope’s essay on this subject, in the London Cyclopædia, without acknowledgment ; which I regret the more, as in the previous edition of this essay, I attributed the plagiarism, very unjustly, to M. Bouillaud, who has merely

¹ *Recherches Anat. Path.* p. 281.

² June, 1833.

³ *Dict. de Méd. et Chir. Art. Pericardite.* (The last volume published in 1834.)

repeated his own florid description written many years ago, instead of having borrowed it, as I had supposed, from the English author. I hope this digression relating to two authors who have written much that is valuable, and to whom we shall have, and have already had, frequent occasion for referring, may be excused on the ground that these facts came in my way incidentally, while investigating my proper subject. At any rate it is proved that both observers agree in the fact of the projection of the walls of the chest. In a case quoted from M. Reynaud, by M. Piorry, the thoracic parietes projected in a space of five and a half inches in length, and three and a half in breadth. The irregular pulsations of the heart and their increased force, are sometimes evident on inspection.¹

By laying the hand on the præcordial region, we may also perceive the changes which take place in the rhythm, force, and extent of the heart's pulsations,² but we learn these characters perhaps more accurately while employing the stethoscope. A redoubling of the second sound, accompanied by a kind of *crackling*, mentioned by M. Bouillaud as a new sign, may be perceived in the same manner.

Percussion.—Laennec had already remarked that in some cases the sound in the præcordial region was flat, but added, that generally, this sign was not very evident.³ The two cases reported by Louis, in his

¹ Bouillaud, *Traité des Maladies du Cœur*. Paris, 1835, Vol. I, p. 454.

² *Ibid.*

³ *Auscult. Méd.* III, 263, 64.

memoir on Pericarditis, in which the diagnosis was founded, in a great degree, upon the presence of flatness on percussion in the præcordial region, and the remarks made by him,¹ were sufficient to produce the belief that this phenomenon would be generally found, if sought for with sufficient accuracy. Four of the cases reported by Andral in the Clinique Médicale, in which the existence of pericarditis was proved by the autopsies,² presented this sign. In the eight cases analyzed by M. Louis, in a lecture in 1833, as all of them presented a prominence in the præcordial region, there can be no doubt that all offered flatness on percussion. In those cases which I have seen diagnosed in his wards as pericarditis, projection of the thoracic parietes and obscurity on percussion have been the two most constant signs, and I believe invariably present. M. Louis considers an effusion of eight ounces sufficient to produce flatness on percussion.³ The two most important signs hitherto mentioned, prominence in the præcordial region, and flatness upon percussion, may be produced also by hypertrophy of the heart. The former of these signs was first observed in hypertrophy, by M. Bouillaud.⁴ We must have recourse, then, to the use of auscultation, and to the date, the manner of invasion, and the symptoms of the disease.

The use of *auscultation* may reveal to us either changes in the frequency, force, rhythm, or sound of

¹ Rech. Anat. Path. p. 281.

² Vol. I, Cases, 2d, 3d, 6th, 54th.

³ Lectures.

⁴ Traité, etc. Vol. II, p. 441.

the heart, or the existence of some unnatural sound superadded to those of the healthy action. In eleven cases, two of which are from the memoir of Louis, and nine from Andral, the action of the heart, as observed by auscultation, presented the phenomena here mentioned.

	<i>Extent.</i>	<i>Frequen- cy.</i>	<i>Force.</i>	<i>Rhythm.</i>	<i>Sound.</i>
Louis.	Impulse some- times in small extent.	—	Unequal, con- fined impulse, occasionally.	Tumultuous, some- times regular after repose.	—
"	—	—	—	—	Dull, distant, often doubtful. Respira- tion absent in car- diac region.
Andral.	—	Very fre- quent.	Irregular.	Tumultuous, inter- mittent.	—
"	—	—	Very feeble.	Confused the two sounds undistin- guishable.	—
"	Heard through- out whole front of chest, some- times regular.	—	—	Tumultuous, irre- gular.	Heard very slightly.
"	Heard in small extent.	—	Without im- pulse.	Intermittent.	—
"	—	—	Impulse slight.	Regular.	(Vague rustling felt by hand.)
"	—	—	—	—	—
"	—	—	—	—	—
"	Natural.	—	Natural.	Intermittent.	—
"	—	Ex- treme freq.	Nothing else remarkable.		

In examining this little table, which I have carefully made out from the original cases, it will be seen that of the nine cases in which the signs given by auscultation were recorded, every one offered some-

thing unnatural. In six there was disturbance of the rhythm, in five of the force, in two of the sound, and in two of the frequency. But on looking at these descriptions, it seems probable enough that the authors were contented to note the most remarkable phenomena found by auscultation, and that they omitted certain less obvious derangements of the heart's action. Thus in the fourth case, if the sounds were confused and undistinguishable, it is probable that both the frequency of the pulsations, and the quality of the sound were more or less altered. Again, the necessity of repeated examinations is illustrated by a circumstance noted in the first case, namely, that the rhythm was sometimes regular after repose. If the observer had been contented with a single examination, and had fallen upon one of these intervals, an important element in the diagnosis would have been wanting. But in addition to the signs detected by auscultation in the cases of Louis and Andral just analyzed, to which we may add the *increased jerking impulse*, and the unusual *sonorousness* of the ventricular systole, on the authority of Dr. Hope,¹—other signs have been added of late years.

In the third volume of the Treatise on mediate auscultation² Laennec mentioned a sound heard in the præcordial region like “the creaking of the leather of a new saddle under the rider.” This sign he had

¹ Lond. Cyc. of Pract. Med. Art. Pericarditis and Carditis. I am rather surprised to see the last of these two characters assigned to pericarditis. M. Bouillaud speaks of the *obscurity* of both sounds in the stage of effusion. (Traité des Mal. du Cœur, Vol. I, p. 458).

² P. 64.

supposed to belong to pericarditis, but he had relinquished this idea at the time he wrote. His clinical assistant, M. Collin, with another student of La Charité declared positively that they had observed this sign in two cases of pericarditis, and the former mentioned it in a thesis published in 1823. However, Laennec said nothing of it in treating of pericarditis in his second edition published in 1826, nor did he mention any other accidental sound as belonging to this affection.¹

In the article Pericarditis, in the London Cyclopædia of Practical Medicine,² Dr. Hope mentions the bellows murmur as constantly accompanying the first sound of the heart when the jerking impulse exists.

The character of the accidental sounds belonging to this affection not being so generally understood as many other of the auscultatory phenomena, I shall give them as stated in the Treatise on the Diseases of the Heart of M. Bouillaud³—condensing his account as much as possible.

The sound of the creaking of new leather he has only recently observed; two or three times within five or six months. But nothing is more common, he says, than to observe a sound of friction or of grazing⁴ more or less characterized. In certain cases the sound resembles that of the rasp or saw, and in six or eight cases he had observed a true bellows sound. To distinguish the bellows sound from the sound of friction, he observes that the last, which is

¹ Auscult. Méd. Vol. III, p. 262.

² June, 1833.

³ Paris, 1835, p. 456, etc. Vol. I.

⁴ *Bruit de frottement, bruit de frolement.*

more diffuse and superficial, is accompanied by a grating, scraping, or rasping motion not found in the purest bellows sound. The sound of friction in pericarditis is analogous, he says, to that found in pleurisy in its cause—that is, it is due to the movement of parts made rough by false membranes, over each other. It is isochronous with the heart's pulsations, more pronounced commonly during the systole than during the diastole; and in some cases resembles the crackling of taffeta, bank notes, or parchment. But the bellows sound he attributes to the swelling of the valves, to the formation of coagula, or to the compression exercised by effusion, either as a primary or an accessory cause. The sound like the creaking of new leather he attributes to the frictions and tractions exerted by the opposed false membranes upon each other, but in this case he supposes they are more dense, elastic and leathery.

M. Louis is in the habit of pointing out the absence of the respiration in a larger extent than natural about the præcordial region.

If the authority of Hope and Bouillaud, the two leading writers on diseases of the heart, is of any value then, auscultation has added new light to the diagnosis of a disease which had baffled Corvisart and Laennec, but which the labors of M. Louis had already so far illustrated that his scholar, M. Legallois, could say in 1830, "At the present day we do not divine, but we diagnosticate pericarditis!"

It is unnecessary to devote many words to *hydro-pericardium*. If the effusion were copious, it would be recognised by signs similar to those of inflammatory effusion, but the absence of false membranes

would prevent our finding the sound of friction which may exist in pericarditis. In cases where there is general dropsy, the examination of the pericardium and pleura should not be forgotten. *Essential hydro-pericardium*, which Laennec considered as very rare,¹ is designated by M. Louis as an imaginary disease.²

In the more exceptional case of the existence of aeriform fluids in the pericardium—*pneumo-pericardium*—Laennec tells us that there is generally effusion of liquid also. He affirms that he has sometimes recognised it by a clearer resonance at the lower part of the sternum, or by “a sound of fluctuation determined by the pulsations of the heart, and by strong inspirations.” The author of the thirtieth case in M. Bouillaud’s treatise (M. Fournet) observed a sound of fluctuation in the præcordial region, which M. Bouillaud, in opposition to the opinion of the author of the observation, refers to the movement of the liquid in the stomach or bowels. As it is perfectly possible that there was aeriform fluid as well as liquid in the pericardium, I can see no propriety in the peremptory assertion contained in the note of M. Bouillaud.³

A sentence of Morgagni, alluding to fluctuation in the pericardium, is remarkable as containing a hint upon *auscultation* which, from the days of Hippocrates to those of Laennec, seems, with this exception, to have been scarcely ever, if ever alluded to. (Note D.)

The signs attributed by Saunders to the adhesion of the heart and pericardium, namely, a depression

¹ Auscult. Méd. III, 272.

² Lectures.

³ Traité des Mal. du cœur. Vol. I, p. 500.

under the false ribs of the left side during the ventricular systole, followed by a little elevation at the same point and below it, during the diastole, not having been confirmed by the observation of the two authors by whom it is mentioned,¹ is probably of little constancy.

A few practical remarks will close what we have to say upon this disease. That it has been frequently overlooked, is proved by the comparative rarity with which it has been diagnosticated during life, contrasted with the frequency with which adhesions between the heart and pericardium are found upon the dead subject—once in twenty-nine cases, according to Louis. That its severity has been overrated is proved by our finding not very unfrequently, universal adhesions in patients dead of diseases other than pericarditis. M. Louis is in the habit of speaking of pericarditis as a disease which generally terminates favorably in patients otherwise healthy. According to his estimate some years ago, it is fatal only once in six times.

The general *treatment* of this affection is analogous to that of pleurisy. M. Bouillaud asserts that by means of blood-letting, carried to a great extent, almost all the cases of pericarditis which he has met with have yielded.² It is obvious that the proper application of our local remedies must here also depend upon our knowing the seat of the disease. If the disease was confounded by the practitioners who relied on the symptoms merely, with bronchitis, with

¹ Laennec, Vol. III, p. 268. Bouillaud, *Traité*, etc. I, p. 467.

² *Traité*, etc. Vol. I, p. 479.

pneumonitis, with pleurisy, in such cases was it to be supposed that remedies would be applied particularly to the præcordial region? What was to become of those cases in which there was no pain, or in which it existed not merely in the seat of the disease, but in other parts also,¹—what was to be the practice of those, in short, for whom this affection had “no group of distinguishing symptoms?” Now, with regard to the use of local bleeding in this affection, men who differ immensely among each other with regard to many things, are agreed—such men as Lermnier, Chomel, Louis, and Bouillaud. The remarks upon the utility of *diuretics* in the stage of effusion in pleurisy, may apply also to this affection. The use of *sedatives*, particularly digitalis, which controls to so extraordinary a degree, the undue action of the heart, will in some cases be indicated, when if the disease had been taken for bronchitis or pneumonitis, we might have been stimulating the patient with acrid expectorants. Nor, as we have said, in speaking of pleurisy, shall we submit the patient to the antimonial treatment, under the idea that we are dealing with an inflammation of the lungs in a manner sanctioned by the best authorities. Finally, if the operation of paracentesis is applicable to the pericardium, its propriety must be established in every instance by the signs afforded by direct exploration.

The history of *carditis* is too obscure to entitle it to more than an allusion in this place. M. Bouillaud, who has been long engaged in studying the diseases

¹ Andral, Cases 2d, 3d.

of the heart, says expressly, "I have never met with a case of carditis which was not complicated with pericarditis, or endocarditis, (inflammation of the internal membrane of the heart), and I own that the signs of these two last affections were all that attracted my attention."¹

Diseases of the Valves of the Heart.

I am aware that M. Bouillaud, the most recent writer on the diseases of the heart, treats of these affections as the result of a pre-existing inflammation, that disease to which he has given the name of endocarditis. But this is not the place to examine the evidence in proof of, or against the truth of this proposition. And with regard to *endocarditis*, it is enough to say, that he assigns as its characters, pulsations of increased violence and extent, superficial and visible, sometimes with perceptible vibration to the hand, rapid, irregular, or intermittent, attended with the bellows sound, and sometimes with a metallic tinkle isochronous with the ventricular systole. According to the same author, this disease often co-exists with pericarditis, cannot always be distinguished from it, and requires essentially the same treatment. A case of this affection may be found in the work of Dr. Gerhard. I do not consider it necessary to follow M. Bouillaud into the detail of the various anatomical lesions of which the valves may be the subjects, or to enumerate with Dr. Hope the minute distinctions which may indicate that the cardiac, or the arterial

¹ *Traité*, etc. Vol. II, p. 302.

valves, of the right or of the left side, are singly or in common affected with disease. The only one of these affections which demands our attention is the induration of the valves with contraction of their orifices, the diagnosis of which, according to M. Bouillaud, is as certain as that of induration and contraction of the rectum or urethra by a skilful surgeon.¹

The application of the hand gives the sensation described by Corvisart as a thrill or an undulation, and by Laënnec in the terms which have been translated purring thrill, or tremor. It makes known also the different irregularities in the heart's action.

Percussion can only reveal the existence of complications of the primitive disease, the different forms of hypertrophy and dilatation.

Auscultation shows the presence of some of the modifications of the bellows, rasping, or sawing sound. M. Bouillaud insists on the constancy of this phenomenon. In more than a hundred cases of disease of the valves, he says, there was only one where he did not find it; and more careful and frequent examinations, he believes, would have detected it even in that instance. The sound which presents one of the characters mentioned may be single or double, sudden or prolonged, feeble, or of sufficient intensity to be heard at a certain distance from the chest.² The general conclusion of M. Bouillaud with regard to the diagnosis, is as follows. "When we find a permanent bellows, or rasping or sawing sound, and the vibrating thrill over the præcordial region; if palpitations, or

¹ *Traité*, etc. Vol. II, p. 213.

² *Ibid.* p. 215.

tumultuous, irregular, intermittent pulsations of the heart exist also, it is almost certain (provided that the disease have lasted several months or years), that there is induration of the valves, with contraction of one or more orifices."

As the differential diagnosis of diseases of the heart was scarcely thought of before the days of Corvisart, who himself employed palpation and percussion, I need hardly refer to those classical writers unacquainted with the methods of direct exploration to show the confusion into which they have fallen. Dr. Good, whose nosology is the best puzzle with which I am acquainted, speaks of several of these diseases under the same head, but in the last place where one would have looked for them, among the "diseases of the nervous function."¹ Atrophy and hypertrophy, inflammation and dropsy, disease of the valves and nervous palpitations, are all jumbled together side by side among the diseases of the nervous function! Dr. Gregory says, "It is, I believe, quite impossible to ascertain with any degree of precision, during life, the existence of diseased valves, as separate from every other variety of disorganization of the heart."²

Although this affection is in itself incurable, yet if we may by prophylactic measures avert some of the consequences of valvular disease, as hypertrophy and dilatation, or at any rate retard their progress, it cannot be a matter of mere curiosity to ascertain an organic lesion which may at one time be mistaken for

¹ Class Neurotica, Order Cinetica, Genus Clonus, Species Palpitatio.

² Practice of Physic, Vol. II, p. 232.

emphysema, and at another for chlorosis. It is peculiarly when contrasted with the latter disease, that the importance of drawing the line of distinction is to be seen; for while certain tonics, and the use of exercise, are almost a specific for the one, if similar symptoms were due to disease of the valves, we should prescribe sedatives and repose. That this distinction is sometimes difficult, may be seen by a case mentioned in the Manual of M. Raciborski, in which he had supposed the existence of an organic lesion, while M. Bouillaud diagnosticated the disease as chlorosis.¹ The last author remarks that this mistake occurs every day, that the similarity of the symptoms renders it easy to confound them, and that he was formerly unable to make the distinction himself, which he has at length traced between them. In chlorosis, he says, there is commonly no well marked bellows sound, but the large arteries, especially the carotids and crurals offer on examination a rumbling, or whistling, or moaning sound, etc.² Within three years, he has observed this phenomenon at least a hundred times.

Hypertrophy of the Heart.

With regard to the uncertainty in the diagnosis of this disease from the general signs, we need not repeat what we have said in speaking of disease of the valves. The principal symptomatic disturbances of the circulating system, are irregularity in the heart's action and change of character in the pulse. But

¹ Nouveau Manuel, p. 294.

² Traité des Mal. du Cœur. Vol. II, pp. 485, 486.

the first of these symptoms may be owing to many other causes besides hypertrophy, and the pulse may be disguised by valvular disease, which frequently complicates this affection, or rather is its cause. The symptoms on the part of the lungs, and some of the principal morbid phenomena of the capillary and secretory systems, are either common to other diseases, or only found at the advanced stage of this.

Those practitioners who do not make use of direct exploration, may indeed declare a patient affected with disease of the heart whom they find suffering with violent palpitations, his limbs infiltrated, his countenance mottled with purple veins, and all his muscles straining to draw in air enough to redden the black blood stagnating in its vessels; but will they venture to assert the existence of hypertrophy, in cases where the only general signs are "a little shortness of breath on exertion, and occasional feelings of slight palpitation?"¹

The physical signs which characterize hypertrophy, are all of them such as might have been anticipated on observing the increased volume and extent of the organ.

M. Bouillaud, I believe, was the first to point out the existence of a projection in the cardiac region.² *Inspection*, which reveals this sign, may also show the increased violence and extent of the heart's pulsations. The point of the heart, according to M. Bouillaud, may be seen to strike the intercostal spaces to the left of its natural position.³

¹ Hope, Lond. Cyclop.

² *Traité*, etc. II, 441.

³ *Ibid.*

Palpation.—The information which this means of exploration affords us, being obtained also by auscultation, it need not be described separately.

Percussion.—The immense increase in the volume of the heart which is sometimes met with, and which has given rise to the expression “cor bovinum,” must evidently increase the natural flatness on percussion, found in the præcordial region. In comparing the maximum weight of the heart in eleven cases of hypertrophy, (688 grammes) with the mean weight of the same organ in fourteen healthy subjects, (350 grammes)¹ we can perceive how far this difference on percussion must sometimes be carried. Thus in the 119th case of M. Bouillaud, the præcordial region was flat on percussion in an extent of from twelve to sixteen square inches.

Auscultation.—According to M. Bouillaud, the pulsations of the heart are not increased in frequency except when accidental causes induce palpitation; they are sometimes even slower than usually, and unless in cases of complication with nervous or valvular disease, they are regular.² According to the same author, the sounds are dull when the hypertrophy is very great, (twelve or fifteen lines or more), and the cavities diminished in size. But when the hypertrophy is moderate, the cavities of natural size, or dilated, he says the sounds of the heart are stronger, more sonorous and clearer, and heard in a considerable part of the chest, even over its posterior surface.

¹ *Traité*, etc. Vol. I, p. 72.

² *Traité*, II, 442.

Dr. Hope considers "a strong, slowly heaving impulse," as the principal sign of simple hypertrophy, and the sudden sinking back of the heart afterwards, which he calls the "back-stroke," as proving the affection more developed. Both these signs may exist in hypertrophy *with contraction*, but less distinctly, and may be absent if the disease is slight. In hypertrophy *with dilatation*, the impulse is sharp and smart, and the sounds are increased; sometimes the ventricular contraction is accompanied with a bellows murmur.¹

When we compare the physical signs of this affection with those of pericarditis, we cannot but be struck with their correspondence. In both there may be flatness on percussion in the præcordial region, increase of impulse, dullness of sound; and the existence of complications may give rise to the bellows sound or similar phenomena in hypertrophy; sounds which we have seen exist in pericarditis. Still, there are points of difference that must often be sufficient to render most probable one or the other affection; as the sound of friction in pericarditis, the tumultuous action of the heart in the same affection, the extent of the projection of the thoracic parietes, the sound of undulation; and in hypertrophy, the regularity of the pulsations and the very great force of the impulse, like the "blow of a fist,"² or the stroke of a hammer.³ But after all, it is to be remembered that the question commonly is, whether a disease is pericarditis or

¹ London Cyclop. of Pract. Med. Art. Hypertrophy of the Heart.

² Bouillaud, *Traité*, etc. II, 441.

³ Hope, Art. Hypertrophy, etc. Lond. Cyc.

sóme other *acute* disease, or whether it is hypertrophy or some other *chronic* disease, because the history of the symptoms is sufficient to decide pretty satisfactorily to which of these two categories belongs the disease in question. Consequently, as no other *chronic* disease offers physical signs like those of hypertrophy, these signs, even if in themselves actually such as might be found in pericarditis, will, and frequently do, enable us to form a just diagnosis.

Is this a matter of any practical consequence? I can only answer this question by referring to the best authorities on these diseases. Laennec, who pursued the rigorous method of Valsalva and Albertini, in conjunction with some other means, says he could cite a dozen cures of several years' standing. Dr. Hope and M. Bouillaud reject this extreme course, but both employ repeated bleeding, general or local, during the treatment, with rest, mild diet, and sedatives. The first of these authors thinks that purgatives and diuretics are useful, even when there is no effusion. Such are the principal means, by the employment of which M. Bouillaud tells us, that in "pretty numerous cases" he has seen "remarkable diminution, if not complete disappearance, of hypertrophy," and the English author has "found it to effect cures in a considerable number of instances, some of which were advanced even to the second degree."

We have referred in the course of these remarks to the case in which *hypertrophy* is complicated with dilatation. The existence of dilatation of the cavities of the heart with *atrophy*, or thinning of their walls, is so rare, that of forty-five cases of disease of the heart, observed by M. Louis, at La Charité, not one

presented it; and in mentioning this fact, he added, that he had not subsequently met with it. We shall therefore not occupy ourselves with this affection.

Aneurism of the Aorta.

The insidious progress of this disease; the insufficiency of the general symptoms to prove its existence, are mentioned in strong terms by Laennec,¹ and by Dr. Hope.² The first of these authors declares, that “frequently the first indication of its existence is death, as sudden as if from a musket ball.” “I have seen,” he says, “men die in this way, who were supposed in the most blooming health, and who had never complained of the slightest indisposition.” We need not add other authorities to those just quoted, for it is evident in itself, that the symptoms must depend upon the point from which the aneurismal tumor is developed, its volume, and the organs on which it presses; and it is also clear, that most of the functional disturbances, occasioned by this cause, as dysphagy, or dyspnœa, or serous effusions, might arise from some other causes.

Dr. Baillie, who has endeavored to enumerate all the established symptoms and signs of the lesions he has described, mentions as the physical sign of this affection, only the presence of a tumor with strong pulsations.³ These pulsations, he observes, are commonly perceptible to the eye when the chest is exposed, but this morbid action he had observed in several other complaints.

¹ Auscult. Méd. III, p. 321.

² Lond. Cyc.

³ Morbid Anatomy, p. 36.

Corvisart considered the diagnosis always obscure if there was no external tumor, and evident when the tumor could be seen and felt.¹ In the first case, he remarks that most of the signs may be confounded with those of other thoracic affections. The other physical signs to which he attributes importance, are, 1. The whistling produced during respiration and in speaking, by compression of the trachea, which may be owing to other causes than an aneurismal tumor; as to disease of a bronchial gland in a case which he reports. 2. A rustling which may be felt in the præcordial region; the purring thrill of Laennec. 3. Obscurity on percussion at the upper and middle part of the chest.

Laennec left this affection among those for which he had found no pathognomic sign. He depreciates the value of the purring thrill and of percussion.² He did not decide how far the stethoscope *might* be found useful in the diagnosis of this affection, but he tells us that facts had proved to him that a voluminous aneurismal tumor might exist without giving any stethoscopic signs. Still, he attached a certain value to the existence of simple pulsations, commonly much stronger than those of the heart, which he thought, however, frequently wanting. But if an impulse isochronous with the pulse, and evidently stronger than that of the ventricular systole as explored in the præcordial region, was heard under the right clavicle, constantly, in repeated examinations, he considered this circumstance as evidence of the existence of an aneurism.

¹ *Maladies du Cœur*, p. 351. ² *Auscult. Méd.* III, p. 323, et sequent.

The article of M. Bouillaud in the *Dict. de Médecine et de Chirurgie* is meagre, and contains little that is new and important. He notices the clearness of the sound of the pulsations in an aneurismal tumor, and believes that Laennec had overlooked this circumstance when he spoke of large aneurisms offering no stethoscopic signs. He remarks that the sound of the aneurismal pulsation may be double, an observation, the truth of which I have seen in a case diagnosticated by M. Louis as aneurism of the aorta, and proved to be so upon subsequent examination. Among the stethoscopic phenomena he quotes M. Reynaud's observation, that hægophony may exist when the trachea and bronchiæ are compressed.

The article of Dr. Hope, in the *London Cyclopædia*, is much more elaborate and complete, so far as regards the physical signs. He begins with the encouraging declaration, "Taking the part of auscultation against its immortal discoverer, we hope to show that there is now little difficulty in the diagnosis of the three affections in question."¹ The work in which this article is contained being extensively circulated in our country, I shall merely mention the signs given by him, referring to the original essay for many valuable details.

The resonance on percussion, he says, is seldom impaired unless the tumor be very large.

The pulsations may be either single or double.

The characteristic signs are,

1. Loudness of the first sound, (if double).

¹ Aneurism of the Aorta, Pericarditis, Concretions in the heart before death.

2. This loudness of the first sound decreases as we approach the cardiac region.

3. The loudness of the second sound, on the contrary (which is only the second sound of the *heart* transmitted to the region of the tumor), increases as we approach the præcordial region.

4. In addition to loudness, the characters of the aneurismal sound are its hoarseness, resembling that of rasping a sounding board, its abrupt beginning and end, and its short duration.

5. The purring tremor above the clavicles, is valuable as a sign of dilatation of the arch, but unfrequent and imperfect in sacculated aneurisms.

6. Pulsation at the points corresponding to the tumor.

The author remarks, that the sacculated aneurism of the abdominal aorta is comparatively so easy of detection that he has not thought it necessary to enter into detail respecting its signs. He does observe, however, that it is to be recognised by a constant, strong impulse; by a loud, brief, abrupt bellows sound, not so hoarse as that of aneurisms in the chest, sometimes audible in the back. The pulsations are single, and we may sometimes, by forcing the stethoscope in different directions down upon the tumor, obtain an idea of its position and dimensions.

Such are the principal signs which characterize aneurism of the aorta. For their exact application in the detection of fallacious symptoms, our limits compel us to refer to the original essay. Whether the confidence which Dr. Hope expresses in them as diagnostic marks be well founded is not easily to be decided by the personal experience of common observ-

ers. I have seen but two autopsies of patients dead of aneurism of the aorta. The first was never thoroughly examined by the stethoscope to my knowledge, but the presence of an immense pulsating tumor in the lumbar region had long revealed the existence of an aneurism, which proved, on examination, of most extraordinary dimensions. The other case was in the wards of M. Louis, and in this instance the existence of a tumor at the anterior part of the right side of the chest, flat upon percussion, offering double pulsations, and the purring thrill, had long induced M. Louis to announce an aneurism of the aorta, which was found true after the fatal termination of the disease. In one of my last visits to his wards I saw two patients in whom he had announced the existence of the same disease, but I do not know the end of their history.

Thus we see that even Laennec left this disease in obscurity, and that if, as we have very high authority for declaring, its diagnosis is now attended with "little difficulty," it is owing to the greater attention paid to the signs afforded by auscultation.

The diagnosis of this disease is important both with regard to the prognosis and the treatment. With regard to the prognosis, for the physician should at least be aware that he is treating a disease which may strike down a patient apparently suffering from trifling symptoms, at any moment, however little anticipated.

The celebrated treatment of Albertini and Valsalva has been more peculiarly appropriated to this affection. To reduce the amount of circulating fluids, to quiet as far as possible the disturbances of the cir-

culatation by regulated habits and repose, are the general indications. The circumstances in which the rigorous method just referred to should be employed, and the modifications it may require, are treated of in the essay of Dr. Hope from which I have borrowed so largely. Life may be long maintained while this deadly lesion is gradually advancing to its fatal termination. In the case to which I referred, of a patient in the wards of M. Louis, the woman had noticed the tumor seven years before her entrance, and M. Louis, who had seen her four years before her entrance, had observed at that period that it was flat on percussion, and presented double pulsations.

AFFECTIONS OF THE ABDOMEN.

There are two reasons why this class of diseases should be considered in far less detail, with regard to their physical signs, than was requisite when treating of those of the thoracic organs. First, because inspection, palpation, and even percussion, have been very long recognised as useful and important in these affections; and secondly, because the use of auscultation has hardly been applied to the organs of the abdominal cavity, with the exception of the aorta, and of the impregnated uterus. We have already spoken of the first, and we cannot consider the physical signs of pregnancy as coming within the scope of our question. If we were to discuss the value of auscultation applied to the diagnosis of this condition, there would be no reason why we should not introduce also a disquisition upon the pelvimeter, and the prac-

tical utility of examinations of the neck of the uterus by the finger or the speculum, and the importance of the sign which the French call "*ballotement*." Now, as all these subjects are treated of in works upon midwifery, and as they relate to a healthy function, however useful and important these signs may be, they must not, evidently, be considered as belonging to medical practice.

Effusion in the Peritoneum.

The information afforded by inspection, and the common method of detecting fluctuation, are too well known to be here insisted upon. It is only requisite to mention the advantage to be derived from two processes less commonly employed.

The first is the "fluctuation peripherique," or, as Dr. Forbes has translated it, "superficial fluctuation," a sign first mentioned by Mr. Tarral, an English gentleman residing in Paris. The process for obtaining this sign, as described in the work of M. Piorry, is as follows. Both hands are to be placed upon the abdomen, two or three inches apart, and so that the two forefingers may be parallel; a slight impulse being then given by the forefinger of one hand, will be immediately perceived by that of the other, if there is fluid to transmit the shock. Some prefer, says Mr. Tarral, another method, which is to place the left hand only on the abdomen, and to strike the parietes with the forefinger of the other hand, obliquely, and as it were grazing the surface.¹ By

¹ Piorry, Proc. Op. p. 137. Forbes, London Cyc. Art. Abdomen, Exploration of.

means of this experiment, Mr. Tarral says he has often detected very trifling effusion in the depending parts of the abdomen.

The other method of exploration is *percussion*, which M. Piorry has described in its details with very great minuteness, in the work to which we have often referred.

The following, according to this author, are the principal marks of moderate effusion which are afforded by percussion.

1. Increased sonorousness in the umbilical region (explained by the specific lightness of the intestines, containing gas which causes them to float above the liquid).

2. Flatness on percussion, presenting a *line of level*, at the lower parts of the abdomen, and becoming more marked in proportion as we percuss lower.

3. The existence of the sound which he calls *humoric*; a sound produced by percussion upon a cavity where aeriform and liquid matters are in juxtaposition.

4. Change of level in the fluid upon varying the patient's posture.

The author owns that much habit is necessary to appreciate the difference of sound in the depending parts, when the effusion is very slight. The flatness is to be distinguished from that produced by matters in the intestines, by the absence of the line of level in the last case, and their not changing place with the patient's position. The experiment should be often renewed; in the right iliac fossa, we should

begin by finding the natural sound of the cœcum, and then turn the patient so that the liquid may flow to this part. If there is œdema of the abdominal parietes, they must be pressed with some force with the pleximeter.

When there is copious effusion, the signs afforded by inspection and percussion, and the presence of fluctuation, leave no doubt upon the nature of the disease.

M. Piorry assures us, that mediate percussion is not painful, if skilfully performed, even in peritonitis. But in this disease, the presence of adhesions and false membranes, or the thickness of the fluid, may so far prevent the displacement of the liquid by change of position, that M. Piorry himself speaks with less confidence than usual of the assistance afforded by his favorite method. Still, he is of opinion that in the majority of cases it will make known the presence of effusion in peritonitis, and as he declares that the general symptoms of this affection are sometimes scarcely manifested, his conclusion must be in favor of the utility and importance of percussion in this disease.

In estimating the value of the two methods thus briefly described as applied to the diagnosis of ascitic and inflammatory effusion in the abdomen, whatever interest they may have as contributing to a minute diagnosis, they appear of altogether secondary practical importance. For ascites is of little consequence in itself, when it requires these subtile processes to detect it, and if it be a symptom of some organic affection, a few days more would probably render it evident to less delicate methods of exploration. Still,

like œdema of the eyelids or ankles, though trifling itself, the very limited effusion detected by the two processes described, may occasionally throw some light upon obscure cases.

In peritonitis, it is so rarely that the common signs and symptoms, when properly sought for, are insufficient, that we have less reason to regret the circumstances which interfere with the certainty of the more novel methods of exploration.

The existence of aeriform fluids in the cavity of the peritoneum, constituting the *tympanites abdominalis* of nosologists, being almost problematical, and at any rate exceedingly rare, we pass to another subject.

Affections of the Liver ;—the Gall-bladder.

I need waste no time in showing that the differential diagnosis of the organic affections of the liver, is impossible by means of the symptoms.

Certain of the changes of structure or form in this organ, may be recognised by means of direct exploration.

The employment of *palpation*, shows us that in many cases of phthisis, the liver passes to some extent beyond the edges of the false ribs, beneath which it is not commonly to be felt. This affords the presumption of its having undergone the fatty degeneration ; a lesion belonging peculiarly to tuberculous disease ; and almost always accompanied with hypertrophy ;¹ so that its presence may become a valuable element of diagnosis in cases where the other symp-

¹ Louis, *Recherches sur la Phthisie*, p. 115.

toms and signs leave it doubtful whether a patient is affected with phthisis.

Sometimes, according to M. Piorry, we may feel the inequality of the surface of the liver produced by tumors. Fluctuation may be sometimes detected over abscesses in this organ.

By means of *percussion*—the limits of the liver may be determined with singular accuracy. With the assistance of lines traced upon the skin with ink or nitrate of silver, we may follow its changes of volume from day to day, as a surgeon follows those of an external tumor. M. Piorry states that he has seen the liver, in a case of jaundice, increase in volume as the yellowness became more intense, diminish as this decreased, and again increase with the yellowness. The patient died; the ductus choledochus was obliterated by a scirrhus tumor; the liver had been exactly circumscribed.¹ I have seen him percuss the liver in a case of jaundice, and announce as the result of his examination that this organ was six inches in thickness at one point, three and a half at another, four and a half between these points, and that the presenting part of the gall-bladder was two inches by one and a half in its dimensions! He allows that deep-seated tumors of the liver may elude the search of the pleximeter, but mentions a case in which an abscess of this organ was discovered by the want of resistance which it offered on percussion.

A peculiar phenomenon which happens on percussing over a cavity containing hydatids—and called

¹ *Procedé*, etc. p. 164.

by him *fremissement hydatique*,"¹ or hydatic thrill, may be looked for in the liver, in which this rare affection is comparatively frequent.

If the affection which led us to explore the liver were complicated with effusion in the pleura, we might still ascertain the extent of this organ, by placing the patient in such a position that the fluid should leave the part of the organ we were examining; but if very great effusion existed, we should perceive that the liver was depressed, without being able to determine its volume. If there were pneumonia with great induration, we must rely on the bronchial respiration and bronchophony in the hepatized portion to distinguish from each other two parts, both of which would be flat on percussion. If there were ascites, we must vary the patient's position while percussing the different parts of the organ.²

The exploration of the liver is important in one other circumstance; that is, when effusion in the abdomen or in the thorax, or meteorism, has elevated or depressed the organ from its natural situation. In the first case, if the displacement were not ascertained, we might be led to suppose that there was hepatization of the lung, or effusion in the pleura, and in the second case, to apprehend the existence of a tumor of new formation. These errors may be avoided by percussing in such a manner as to circumscribe the limits of the liver as accurately as possible.²

According to M. Piorry, congestion of the liver

¹ *Procédé*, etc. p. 37, et seq.

² Vide Piorry, *op. cit.* p. 162.

and hepatitis are accompanied by increase of volume in this organ. As these two affections require the use of blood-letting, and as the increase of volume may be determined by percussion, he concludes, that "the treatment of diseases of the liver receives important light from this method."¹

The practice of opening abscesses in the liver has long been established in surgery ; but if the physician does not suspect the nature of the disease, the patient will not have the chance of benefit from the operation ; it is therefore of some consequence that he should use those methods of exploration which alone can show its existence.

"The fact of recovery in a case where there existed a cyst filled with acephalocysts (hydatids), in consequence of its being opened by M. Recamier, proves of what utility the knowledge of the hydatidic thrill may be in therapeutics."²

Finally, in the case of displacement of the liver, especially from meteorism, we are prevented from committing serious errors in the treatment by understanding the true cause of the morbid phenomena.

Enlargement of the Spleen.

The two diseases in which this circumstance has been most frequently detected, are typhus and intermittent fever. (Note E.)

The size of the spleen is increased in a large proportion of the cases of typhus. Thus, in seventeen of Louis's forty-six cases, it was three, four, or five

¹ Ibid. pp. 167, 68.

² *Procédé*, etc. p. 41.

times more voluminous than natural, and in all but ten of the others, double, or more than double its natural volume.¹

In almost every case of Typhus, according to Chomel, this organ is increased in size, and in about half his own cases he had found it doubled.² The same author has not observed any remarkable difference in the size of the organ in those who died after some days of disease, and those who died a little later.

In cases of intermittent fever, enlargement of the spleen is very common, if not universal; an author, whose name I do not recollect, has even considered the disease as due to this lesion. In two cases, seen by M. Piorry, it was distinctly observed, that during the access the spleen was two inches larger and longer than during the intermission. After the use of the sulphate of quinine, a great diminution was observed in its dimensions, a diminution which did not appear to follow the use of blood-letting or abstinence.³

We are very frequently able to detect the projection of the spleen beyond the false ribs by means of *palpation*. But it is remarked by M. Piorry, that this organ is sometimes developed in an upward direction, so that it eludes this method of exploration.⁴ In such cases its increased volume may be detected by percussion. In making the examination it is important that the patient do not take food or drink for

¹ *Traité de la Fièvre Typhoïde*, I, 289.

² *Leçons de Clinique Médicale (Fièvre typhoïde)*, p. 264. Paris, 1834.

³ *Procédé*, etc. p. 178.

⁴ *Op. cit.* p. 177.

some time before, in order that the stomach may be clearly distinguishable by its resonance, which could not be the case were it distended by liquids or solids.¹ M. Piorry owns that when there exists pneumonia or pleurisy (of the left side) or ascites, it may be difficult to circumscribe the spleen.

The importance and utility of ascertaining the enlargement of the spleen, consist in its application as an element of diagnosis. In very many cases of typhus fever it may be felt with the greatest facility, as I have often had opportunities of observing. To those who have seen the obscure general symptoms and progress of this affection in many cases, this simple additional sign will suggest its own value. For the same reasons percussion will be of assistance when this method is insufficient; and it is to be remembered that typhus fever being one of the two acute affections² in which meteorism is commonly found as a symptom, we must expect that the spleen will be often pressed upwards, so that it would be felt with difficulty, even if enlarged. The signs produced by enlargement of the spleen must be seldom required for the diagnosis of intermittent fever; they might sometimes, perhaps, serve to distinguish the quotidian form from the hectic paroxysm.

Two circumstances are mentioned by M. Piorry as rendering exploration of the *kidneys* by percussion of little value; its difficulty, and the fact that their gravest diseases are generally unaccompanied by hypertrophy.

¹ Op. cit. p. 177. ² Typhus and peritonitis. (Louis, lectures).

Diseases of the Stomach and Intestines.

Changes in the volume, consistence, form, or situation of the stomach may be recognised by the aid of inspection, of palpation, or of percussion. A case which I formerly witnessed illustrated the application of all these methods of exploration. The patient, a woman of fifty-three years, had been suffering from disease, for five months. The principal local symptoms had been vague pains in the abdomen, eructations, vomiting some hours after eating, becoming more frequent, and amounting sometimes to six or seven pints in a day. The form of the stomach could be traced by *inspection*, forming a relief on the walls of the abdomen, extending beneath the umbilicus and even as low as the immediate neighborhood of the anterior superior spine of the ileum. By *palpation* a tumor was detected in the situation of the pylorus. On *percussion*, the prominence supposed to correspond to the stomach was at one time flat, and at another resonant. A movement of fluctuation was easily impressed upon its contents. At another time it evidently changed form while we were looking at it, being at one moment divided into two sacs, as it were, by a partial contraction in the middle, and then resuming the common outline of the stomach.

When this patient was examined after death, the stomach was found so dilated that it occupied almost the whole anterior part of the abdomen, its muscular membrane being hypertrophied, as is usual in such cases, phenomena due undoubtedly to a change of structure about the pyloric orifice, which scarcely allowed the little finger to pass through it.

But it is not always that the diagnosis of cancer of the stomach has been fixed with such accuracy as in this case. The disease which occasioned the death of Napoleon, or contributed much towards it,¹ in which the stomach was extensively ulcerated, without the presence of any voluminous tumor, displayed in no very favorable light the powers of diagnosis either of his attendants, or of the consulting physicians of Rome and London.²

M. Andral has recorded his opinion, that except when there is a tumor which can be felt through the external parietes, there is no certain sign by which we can distinguish that which is called in common medical language, cancer of the stomach, from that which is called chronic gastritis.³ M. Bouillaud, although he attaches more value to the symptoms than the author just cited, depends in a considerable degree on the presence of a tumor for the diagnosis, and allows that there is no other certain sign, when the disease does not affect one or the other of the orifices.

The signs which reveal the accumulation of air in the intestines, *tympanites intestinalis*, or meteorism, are so familiar to every one that we might dismiss them with this allusion. The prominence, the tension, and the resonance of the abdomen are so striking that the most careless observer could hardly over-

¹ Tubercles and excavations were found in one of the lungs; suppuration in some of the bronchial glands, &c. &c. Vide Broussais, *Exam. des doct. Méd.* III, 331, etc.

² Vide Broussais, *Exam.* III, 304.

³ *Clin. Méd.* IV, 107.

look them. I remember when I was a child seeing an animal dead of some obstruction in the intestines, upon whose body, with my companions, I performed *inspection*, *palpation*, and *percussion* (by means of kicks), without dreaming that I was making a scientific diagnosis in declaring that the creature's bowels were full of wind.

The presence of hardened fæces in the intestines has sometimes led to the erroneous supposition of the existence of tumors, or given rise to the symptoms of nephritis, or of sciatica, or to œdema.¹ M. Raciborski, who presented a thesis on the subject of stercoral concretions to the faculty of medicine of Paris, tells us that the true cause of these various symptoms may be detected by means of the physical signs. Sometimes the concretions produce an external prominence, which of course may be seen and felt. By means of percussion upon the tumor and the surrounding parts, we may distinguish the flatness and resistance of the one from the sound belonging to the neighboring organs.²

M. Chomel attaches some value to a gurgling produced by sudden pressure in the right iliac region as a diagnostic sign in typhus.

The importance of ascertaining the existence of organic disease of the stomach, is rather in preventing us from employing improper remedies, under mistaken ideas of some disease which requires active treatment, than because it offers any peculiar indications. No

¹ Raciborski, Manuel, etc. p. 224.

² Ibid.

better illustration of the wild empiricism with which an incurable disease may be treated, can be found, than in the medical history of the prisoner of St. Helena. Half the resources of the *materia medica* were promiscuously applied to his irritated organs, comprehending different forms of tonics, anodynes, stimulants, purgatives, emetic preparations, anti-emetics, and antacids. Well might he exclaim, "*Laissez moi avec vos médecines ! Je vous ai déjà dit cent fois qu'elles ne me valent rien ; je connais mieux que vous ma maladie et mon temperament.*"¹

As we have had occasion to mention already, *meteorism*, according to M. Louis, is peculiar to two acute febrile affections, typhus and peritonitis. With reference to the diagnosis of the first, it is frequently valuable, and it adds certainty to that of the second, which could hardly be confounded with typhus. M. Louis suggests the employment of alkaline or mucilaginous enemata, or of magnesian water as a drink, in cases of meteorism, with the hope of producing the absorption of a part of the gases, and perhaps of acting favorably on the mucous membrane, if that is the source of these gases, or on the matters with which it is in contact,² (to prevent their development).

The obvious indication presented by determining the existence of stercoral concretions, is to get rid of them by means of enemata or purgatives ; an indication very different from that which would be offered were the tumor taken for an abscess,³ or the symp-

¹ Exam. des doct. Méd. III, 526.

² Rech. sur la Fièvre Typhoïde, II, 520.

³ Raciborski, op. cit. p. 223.

toms occasioned by it confounded with those of other affections.

Exploration of the Bladder.

The state of the intelligence being often such, during disease, that the patient is unable to give information with regard to the state of the urinary function, and the use of the catheter being an operation which should be avoided merely as a test, if we have simpler methods, it becomes proper to explore the hypogastric region by palpation and percussion. The first of these methods is generally acknowledged useful, and is employed. The circumstances which, according to M. Piorry, render it often inapplicable are, the distention of the intestines by gas,—great sensibility of the parts,—obesity,—infiltration of the cellular tissue,—or flaccidity of the bladder, even when it contains much liquid, as occurs in old paralytics.¹

In such cases M. Piorry recommends practising percussion. This should be performed from above downwards, on the median line, and at the sides; for the bladder, according to the same author, is often developed in this direction. The percussion should be at first slight, afterwards with more force, and continued as far into the pelvis as possible. The signs which indicate the position of the bladder, are flatness on percussion over the organ, the *humoric* sound at its edges, and tympanitic resonance of the intestines around it.

¹ Piorry, *Op. cit.* p. 195.

The operation indicated by the accumulation of urine in the bladder, is often, as every one knows, both useful and important.

Tumors of the Abdomen.

The large tumors which affect some of the abdominal organs, as the mesentery and ovary, are often obvious both to inspection and palpation. M. Piorry remarks, that in percussing these tumors, the finger meets a degree of resistance which may give some notion of their degree of solidity and their structure.¹ The circumscription of a tumor by means of percussion, sometimes determines that it is independent of an organ to which it was supposed to belong, as in two cases reported by M. Piorry.² The same rules should be observed in exploring abdominal tumors as are directed for ascites; the results, of course, will distinguish the two cases from each other—a distinction of importance, on account of the utility of active medical or of surgical treatment in the case of liquid effusion.

According to Chomel,³ when a tumor is found in the abdomen of a woman, rounded, smooth, and having begun on one side of the abdomen, which it has subsequently filled up, and presenting fluctuation, this tumor is due to the development of a cell of the ovary. If there is little or no fluctuation, if the tumor is irregular, as if by the agglomeration of several others, it is multilocular, and the cells contain liquids of different degrees of consistence.⁴

¹ Op. cit. p. 150.

² Lectures.

³ Ibid. pp. 152, 153.

⁴ Ibid.

It is evident that the prospect of advantage from paracentesis must be much more considerable in the first case than in the second. By employing this remedy in the proper cases, "the life of the patient may be prolonged, and considerable ease and comfort may be thus obtained under a complaint which sooner or later must terminate unfavorably."¹

The existence of air or liquids in the cavity of the unimpregnated uterus is so far from being common, that we need only say that the existence of these conditions may be ascertained by palpation and percussion, and that the indication is clearly to produce the discharge of the fluid, by the introduction of the finger or of a trocar.

DISEASES OF THE BRAIN.

The accumulation of large quantities of fluid in the cavity of the cranium may be appreciated by inspection and mensuration. I am not aware whether the attempt has been made or not in these cases to obtain fluctuation, by the common process, or by that of Mr. TarraL Dr. Fisher, of this city, published in the Medical Magazine for September, 1833, a paper in which he announced the discovery of a phenomenon to which he gave the name of the *cephalic bellows sound*; a sign detected by applying the ear or the stethoscope to the surface of the cranium. Dr. Fisher has observed this sound in cases of hydrocephalus or inflammation within the cranium, and in children during the process

¹ Dr. Lee, Lond. Cyc. Art. Diseases of the Ovaria.

of dentition ; in which last case the sound is more like that of the rasp, or file, than the bellows. The same sound was also remarked in two instances of hooping cough, during the paroxysm of coughing. Dr. Fisher attributes this newly-observed physical sign to some impediment to the flow of the blood through the arteries at the base of the brain. The same gentleman informs me that he has recently met with a *musical* bellows sound on applying his ear to the head of a teething child, and that in two recent cases of apoplexy, upon applying his ear to the head, he has perceived an impulsive sound, resembling that produced by tapping upon the inflated cheek. It is gratifying to perceive that this very interesting discovery is exciting attention in Europe ; and whether it be ignorantly or unfairly that a recent English writer has brought it forward as his own, we may thank him for again directing the minds of observers to the subject, and science may gain indirectly from this apparent attempt at plagiarism. To what extent these discoveries may be rendered practically useful we cannot yet say, but there is certainly reason to hope that they will afford assistance in the diagnosis of cerebral disease, and thus add another to the numerous triumphs of direct exploration.

NOTES.

Note A. p. 249.

WE need only allude generally to a few additional signs revealed by direct exploration ; as change of color and temperature over an inflamed part, œdema, sensibility to pressure or percussion. Among the methods of investigation which have not been generally adopted, are the abdominal pressure of Bichat, and the pulmometry of Mr. Abernethy. The sphygmometer, or pulse measurer of M. Herisson, is a new invention, the value of which has not, to my knowledge, been determined. A curious passage of Avicenna has been erroneously stated by Dr. Forbes. (See London Cyclop. of pract. Medicine, Art. Chest, Explor. of.) He mentions it in this manner. "For this purpose he proposed to apply wetted cloths upon both sides of the chest, and concluded that the side on which the cloth dried soonest, on account of the greatest heat, was that in which the inflammation existed." The experiment would be less dangerous to the patient, and less tedious to the physician, if instituted in the manner related by the Arabian author. "*Et hominum sunt quidam qui ponunt super pectus et latera ejus filum lineum infusum in luto rubeo dissoluto in aqua, et considerant locum qui prius exiccatur: quoniam ipse est locus purus.*" (Liber Canonis, etc. Venetiis, 1490, Lib. III, Fen. x, Tract. IV, Cap. 17.) If the very ancient translation from which I quote is correct, it is not true that he *proposed* this experiment, but he *mentioned* it as having been performed by *others* ; he did not say *wetted cloths*, but a *thread or string* dipped in a mixture of red clay and water ; and the side on which it dried soonest was not "that in which the inflammation existed," but the side *free from disease*.

Note. B. p. 249.

Percussion was made use of by the ancients in the case of *Tympanites*, as its name indicates. "Tympanias autem, ultra tumoris spec-taculum etiam auditu sonorus est; nam ad palmæ percussum abdo-men sonitum edit." (Aretæus De sign. et caus. diut. morb. Lib. II, Cap. 1.)—"Si verberetur superior venter, instar tympani sonitum edat." (Paul. Æginet. Lib. III, Cap. 48.)

A passage in Hippocrates, which seems to have been fated to misinterpretation, contains a comparison of a sound taking place in respiration to the crackling of parchment; a similitude often used in modern days for the crepitous or sub-crepitous rattles. We give the passage as found in three editions.

<i>Και τρίζει το πνευμα οιον μασθλης.</i>	} Vanderlinden, II, p. 83.
<i>Et sanguis stridet velut pellis.</i>	
<i>Και τρίζει το αιμα οιον μασθλης.</i>	} Föes, Lib. II, Sect. V,
<i>Sanguis velut corium stridet.</i>	
	p. 482.
<i>Cutis velut corium stridet.</i>	} Haller, Artis Med.
	Principes.

Stoll has quoted the Latin version of Vanderlinden without any commentary. (Ratio Medendi, Part I, p. 97).

The Greek version of Vanderlinden is evidently the correct one; the *breath* crackles like a skin,—as it may be translated. Any other interpretation is unintelligible. It follows that the Latin translation of this author is incorrect; indeed, it is impossible to translate *πνευμα* by the word *sanguis*; that both the Greek and Latin of Föes are erroneous, and it is probable that Haller, not knowing what to make of the passage, put in the word "cutis" at a venture.

Note C. p. 269.

As a means of increasing the vocal resonance, as heard by auscultation, I may suggest the plan of making the patient speak into a tube, closed at the extremity. I have never, as yet, tried this experiment except upon healthy subjects, and in them the vocal resonance was very evidently increased. Might it not render certain delicate morbid phenomena, as hæmophony, or slight bronchophony, more distinguishable?

Note D. p. 335.

The sentence of Morgagni, to which we have referred, is to be found in the second book, sixteenth epistle, twenty-fourth paragraph. "Et Galenus quidem, ut supra vidimus, scripserat, palpitationem cordis, quæ a pericardii hydrope fit, fieri cum significatione quadam, quod in humore cor ipsum moveatur, liberum nobis relinquens interpretari, an ea significatio ab ægris percipienda sit, ut paulo ante dictum est accidisse nonnunquam, an a Medicis qui ad pericardii regionem, manum *auremve* admovendo aliquid ibi fluctuationis animadvertant, egregium utique præ cæteris signum futurum, et pro pathognomico habendum, si quidem semper, et in iis quoque esse posset, quibus aut pericardium non multum aquæ adhuc habet, aut cor in aqua non vehementer agitur, sed languido et obscuro motu vix contremiscit, sique nulla se interponere aliquando posset fallacia aut ab aquis in thorace stagnantibus, aut ab ipso palpitationis motu, aut a causa alia aliqua Medicum facile decipiente."

Note E. p. 358.

Hippocrates had noticed the enlargement of the spleen in remittent fever, as may be seen in the treatise on Epidemics. (Popularium I, sectio 3.—Vol. I, p. 673, Vanderlinden). "Splen elevatus est orbitali gibbositate. Sudores frigidi perpetuo. Exacerbationes diebus paribus." (Case of Philiscus.)

